

Contemporary Psychology

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Contemporary Psychology

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Psychology on Display

Ernest Havemann

The Age of Psychology. New York: Simon and Schuster, 1957. Pp. ix + 115. \$3.50 (cloth); \$1.00 (paper).

Reviewed by JOSEPH M. BOBBITT

Twelve years ago Dr. Bobbitt became Chief of the Professional Services Branch of the National Institute of Mental Health at Bethesda, Maryland, and now he is Assistant Director of the Institute. That came after a normal academic decade of teaching and research in psychology. He is deeply concerned nowadays with the improvement of mental health, operating in the faith that there are ways to this goal other than the clinical: the parents, the family, the school, the other social institutions can all provide avenues for making happier and stronger the child who becomes the man. Bobbitt wants to help the public to improve itself.

A VOLUME that promises much and offers little.

The promise lies in its title and the area it attempts to cover. It is quite significant that psychology has become of enough interest and importance to be summarized in a popular book for the general public. It is even more significant that the material, in essentially the same form, appeared originally in a *Life* magazine series, indicating that the editors consider psychology good copy for the very large readership of this publi-

cation. In other words, the intention of this publication is to popularize what psychology is and what it means to the average man. It is regrettable that the objective is not reached. The job of telling the American people what our science and profession are all about is yet to be done. What Hogben has done for mathematics, or Gamow for mathematics and astronomy, is not accomplished for psychology in this book.

The promise of the volume also lies in the author. He is an excellent craftsman and a very successful journalist. *Life* identifies him as one of its six top staff writers and as a former associate editor of *Time*. The volume, as might be expected, reads easily and holds interest. What is said is clear and concise. The author is not casual about his topic. He was once a graduate student of psychology and acquired a master's degree from Washington University in 1934, but he forsook his plan for a doctorate to enter journalism. The assignment by *Life* to do the present writing job was made without knowledge of his previous interest in the field of psychology, but there is evidence that he took the job seriously and identified strongly with his mission.

The book offers little because it fails to achieve its purpose for a variety of reasons. In the first place, it is, in many respects, peculiarly out of date. The "age of psychology" that is covered in this volume appears to have ended about 20 to 25 years ago. This statement is especially true if one thinks about the coverage of the discipline of psychology as distinguished from the coverage of some areas of psychological technology and of the field of psychiatry which are also encompassed in this volume.

Perhaps the best example of this point is the treatment of the field of learning. Learning starts with Pavlov who is described as "the first great experimenter in learning." One wonders what happened to Ebbinghaus and a few other early and distinguished students of learning. Conditioning thus gets a little space. John B. Watson makes the book on the basis of his claim that he could make any healthy baby into almost any kind of adult. Trial-and-error learning is mentioned but not described, and Thorndike is not named. The trick of Köhler's ape in getting the long stick with the short stick and ultimately the banana gets pretty complete coverage, but just why the demonstration of insight learning was extremely important to psychology at that juncture is ignored. Learning as a systematic problem of psychology is pursued no further.

The ability of simple organisms to learn, transfer of training, retroactive and proactive inhibition, plateaus in the learning curve, spaced versus unspaced learning, and other learning phenomena

are mentioned in a strange and unordered concatenation. No mention appears of modern learning theories or of their proponents. There is no serious discussion of any of the systematic problems of learning, and the whole discussion of learning could be handled with material already in the textbooks encountered by college students in the mid-1930s.

If the coverage is out of date, the book is equally spotty and out of balance. Learning, sensory psychology, interviewing, industrial psychology, testing procedures, and motivation research are among the areas to which attention is given, but there is no indication that the coverage or emphasis is based upon any assessment of relative importance to the field of psychology. Sensory psychology gets a fair amount of space. One reads about Stratton's glasses and pseudophones, but, unless one has studied psychology, they still appear as cute tricks of experimental psychologists with no particular significance. Testing gets a fair amount of space and is reasonably well handled. Some of the ethics of test production, including the necessity for validity studies before distribution of a new instrument, are discussed. Motivation research is one of the more modern topics considered, and the author even worries a little bit about whether the use of psychology in advertising is quite cricket. The reader, however, is finally assured that he need not worry because (1) psychologists are not really very good at influencing people and (2) not all of them are trying to push us in the same direction (which means that they work for competing companies).

As already noted, the book also covers psychiatry. One wishes that the author had made this inclusion upon some stated rational basis. There is no suggestion, however, that psychiatry is included because it is a part of the generic study of behavior that can be handled as a part of psychology, either as a matter of convenience or for reasons which would appear justifiable to many persons. One gathers, on the other hand, that psychiatry, and particularly psychoanalysis, appear to be particularly good



ERNEST HAVEMANN
With Russian children, Moscow, 1958

copy, and one finds about 40 per cent of the book devoted to psychoanalysis, other psychiatric approaches, and mental health!

The handling of psychiatry is pretty frustrating—a 19-page description of Freudian theory followed by a 27-page assessment of the effectiveness of psychoanalysis, including some words to the effect that few psychoanalysts are today hirsute, extremely rich, or really available. This reviewer is reluctant to attempt critical assessment of the author's handling of psychiatry, but it should be obvious that the task attempted in the space available is an impossible one, and the account cannot get very far in telling people what psychoanalysis and psychiatry are really about. The author's ability to write almost betrays him, because he makes Freudian theory sound quite simple and believable, and one wonders why it takes bright people so long to become psychoanalysts. The assessment of the effectiveness of psychoanalysis gets nowhere; essentially it ends up by saying that many analysts feel they help people but believe that the problem is not one for scientific investigation. This last

point is one which psychologists have repeatedly heard from the analysts, but they have resisted it and obviously they will continue their resistance.

The last chapter deals with other approaches to mental health but is really a strange congeries of the odds and ends of psychiatry. Selye's work on stress gets major attention. The up-grading of mental hospitals in this country in the last decade, and the emergence of the psychiatrically important drugs, among other things, are reported. What this reviewer missed was any appreciation upon the part of the author of the real developments in mental health in the last decade. The author appears unaware of the fact that the prevention of illness and promotion of mental health are becoming today reachable objectives and that the task involved goes far beyond the clinical domain, encompassing social psychology and the other social sciences in their most sophisticated possibilities. None of the research on rehabilitation of mental patients, problems of the aging population, the mental-health effects of school experience, child development, and other hopeful approaches to the improvement

of the mental health of our people is covered in this book. Perhaps the big disappointment, therefore, lies in the failure of the author to show that psychology and its related disciplines are now emerging as areas of study basically related to the everyday problems of everyday people, whether they be clinically ill or not. Is the reviewer here expressing mainly his own interests and bias? Not wholly: he is also reporting his conviction that information about present and past contributions of scientific psychology to this area of human welfare are important.

In summary, the reviewer believes that the book will be unsatisfying to many psychologists. Possibly the average man will learn something from it, but he will not learn much about psychology as a serious scientific discipline. Nor will he gain perspective in the understanding of behavioral science, nor learn much about what is happening in psychology today or during the last five or ten years.

Blind Children Are Competent

Miriam Norris, Patricia J. Spaulding, and Fern H. Brodie

Blindness in Children. Chicago: University of Chicago Press, 1957. Pp. xv + 173. \$3.00.

Reviewed by EUNICE L. KENYON

who has been working with the blind for a decade and more than half that time has been Executive Director of Boston Nursery for Blind Babies. She is especially interested in blind children and in emotionally disturbed children, and she does not think the former need to be the latter.

THE findings of an intensive study of 66 blind preschool children over a five-year period, as reported in this book, represent a significant contribution to our understanding of blindness in children. The longitudinal study was conducted conscientiously and the find-

ings reported objectively. The book is well written, presenting its major conclusion—that blindness, in itself, does not prohibit normal developmental progress—with adequate, well-documented evidence. The authors then succeed in presenting further material which, upon analysis, allows them to isolate certain factors in the child's environment which can and do influence his mental, physical, social, and emotional growth. While some readers may feel that certain of the data are 'over-interpreted,' even this apparent liability becomes an asset when one recognizes the soundness of the recommendations which evolve from these conclusions.

The book should be of particular value to professional workers whose contact with blind children has been limited. The physician will be reassured by the evidence that disturbed functioning of the central nervous system does not necessarily follow in children who are blind due to retrolental fibroplasia. The psychologist will find helpful suggestions to assist him in his efforts to evaluate the blind child, as well as a necessary note of caution against premature conclusions concerning the child's ultimate potential. The social worker and the educator will find evidence for the tremendous importance of their work with these children and their parents. Much can be abstracted from the case material, from the conclusions and recommendations which will be of real assistance to teachers and social workers in their direct work with individual children and parents.

This book should be of particular value to child-guidance workers, nursery-school teachers, and others in demonstrating that such community resources can be of as great value to blind children as to children without major handicaps and that little or no change in procedure is required to adapt such services to the needs of blind children.

This reviewer believes that *Blindness in Children* is among the most valuable studies concerned with young blind children that have been published to date.



And time remember'd is grief forgotten.

—ALGERNON CHARLES SWINBURNE

A Factor Omnibus for Personality

Raymond B. Cattell

Personality and Motivation Structure and Measurement. Yonkers-on-Hudson: World Book Company, 1957. Pp. xxiv + 948.

Reviewed by CHARLES HANLEY

who is Assistant Professor of Psychology at Michigan State University. He has a four-year-old PhD from the University of California at Berkeley. He has been concerned with testing and the development of personality all along and is currently involved, he says, "in hunting for order in the graphite deposits left in IBM answer sheets by college Sophomores when stimulated by personality questionnaires."

CATTELL's is a progress report that represents his efforts to bring a semblance of order into the undisciplined field of personality. It is an omnibus of ideas regarding traits, attitudes, drives, conflict, fatigue, anxiety, typology, personality structure, test theory, plus applications in clinical and industrial psychology.

The author approaches his topics with a firm belief in factor analysis as the preferred method of organizing data, and an unwavering allegiance to the principle that oblique simple structure is the essential characteristic of dimensions worthy of scientific attention. His innovations almost always concern testing and are designed to obtain precise evidence on theories derived from Freud, Jung, McDougall, Kretschmer and lesser figures, from general psychology, and from his own insights. His position on personality theory is thus both eclectic and conservative.

The fundamental dimensions for describing personality are found by factoring batteries of rating and questionnaire scales. A parallel approach with motivational variables reveals factors relating to attitudes, drives, and psychological states. Interrelations of factors are treated as are possible causal links

among them. Cattell has something to say about every facet of personality. His effort, however, is more to harmonize than to reject competing points of view, except when questions arise regarding the identity of basic dimensions and their measurement.

The most novel material deals with new testing devices. Unlike conventional instruments, Cattell's *Objective Tests* measure such aspects of behavior as latency, physiological response, accuracy, and fluency, in the presence of stimuli with 'personality' content. While factors derived from these tests seem less solidly established than those from ordinary scales, readers will share with Cattell the hope, if not the conviction, that the new devices make an important contribution to personality study.

THE book summarizes a host of technical and theoretical publications (stemming mainly from Cattell's laboratory at the University of Illinois, where he is Research Professor). It has as well the character of an advanced text and a proselyting tract. The energy, audacity, and enthusiasm displayed cannot fail of admiration, yet it is an unfortunate fact that Cattell, the writer, is hardly the equal of Cattell, the psychologist. The simplest material is made extraordinarily difficult while, paradoxically, some of the mathematically demanding sections are clear and neat.

The chief difficulty in the writing lies not in the neologisms, the technical jargon, and the Graduate-School English, but rather in details that careful revision could have eliminated. Cattell can write a straightforward sentence ("Although technology is not science, the existence of a flourishing technology often indicates the existence of a highly developed basic science"). He has mastered the sly dig ("It may turn out that an unexpected, but appreciable, psychological contribution from this work will be education in a sense of humility about our conceptual prowess as psychologists, and a realization that factor interpretation, deduction, and hypothesis formation call for more subtlety, respect for empiricism, and conceptual penetration than have customarily been exercised in arriving at conclusions in



RAYMOND B. CATTELL

the clinical field"). It is in the paragraphs that matters get out of hand.

Cut and dried tasks, such as the description of each factor, are confounded by the intrusion of hypotheses, observations, anticipations, and postponents. At its worse, the narrative becomes an alphabet soup so thick a parenthesis drowns:

"Among psychotics and mental hospital outpatients, Lorr's AD in outpatients (407) and E in psychotics (406) best fit our F, though his A in psychotics (which we have matched with our D or U. L. (L) 4 has considerable resemblance too. Lorr matches his E with Degan's B, with which match we agree, but the 'agitated depression' pattern of Wittenborn we are more inclined to match with Lorr's factor of 'anxiety' as shown below. (See Factor O, next chapter.)"

This writing cannot be praised; the content is another matter. Cattell's modern conservatism will be admired in proportion to a reader's pleasure in finding giants and dwarfs living together under one roof. Psychologists who like heterogeneous results painted on a broad canvas will be delighted with Cattell's achievement; those who wonder how the brushwork was managed will find little to go on. (Only a few 'captured' studies are treated at length, one being Kelly's investigation of age changes in personality, the Presidential Address of the American Psychological Association in 1955. Narrow-gauge psychologists may balk at the manner in which self-ratings on Physical Energy are trans-

lated as Surgency, Voice Quality as Cyclothymia, Breadth of Interest as Comention. The fact that graphs illustrating changes in test-retest correlations over time are scaled to square roots of coefficients, instead of squares, as the text asserts, does not help.)

The Objective Tests should arouse great interest, but their description is sketchy. The chapters on clinical and industrial psychology are full of ideas, but evidence of their utility is presented in findings of 'statistical significance' rather than 'amount of criterion variance accounted for.'

A GENERAL problem arises from Cattell's investment in the linear 'specification equation': $R_{ji} = s_{j1}T_{1i} + s_{j2}T_{2i} + \dots + s_{jn}T_{ni}$; where R_{ji} is a given behavior, the T s are an individual's factor scores, and the s terms weights for factors in the particular situation. Readers who dimly remember the curved and wiggly lines in natural science textbooks may wonder why human behavior is so simple. Cattell ignores this question for nearly 400 pages, then admits that more complex relationships are possible. After a short section describing a few such equations, he pulls the rug from under himself:

"The shrewd reader may interject at this point: 'If the simple specification equation is abandoned, so also is the factor method of finding functional unities, for they are mutually interdependent, factors being consistent with linear relationships.' This is logically correct; but . . ."

Then follows a chain of lawyer's arguments: new methods might be discovered (but nothing much is to be expected from them), the linear model always approximates complex functions, skillful manipulation will uncover such cases, and there are no S-shaped relationships anyway. Strict adherence to Cattell's position would seem to guarantee overlooking nonlinear relations, whereas any stable equation involving test and nontest behavior is welcome in science, regardless of one's preference for straight lines.

Personality and Motivation Structure and Measurement is aimed at informing and converting psychologists, and its author hopes it will be used as a text

for graduate students and undergraduate majors. (For this reason, a list of blood-curdling problems follows each

chapter.) Readers are bound to be simultaneously stimulated and irritated by this book.

A McGuffey for Mother

D. W. Winnicott

Mother and Child: A Primer of First Relationships. New York: Basic Books, 1957. Pp. xii + 210. \$3.50.

Reviewed by BETTYE M. CALDWELL

Dr. Caldwell was until August, 1958 Director of the Child Evaluation Clinic of Washington University's School of Medicine in St. Louis, where she engineered an inter-Disciplinary (she says the D is big) clinic for mentally retarded children. She has been a developmental psychologist ever since she was exposed to R. R. Sears at Iowa in the mid-1940s, concerning herself with everything from neonates to geronts (not the merely mature though; they look after themselves). She is also doing research on the effects of parent attitudes on the adjustment of handicapped children.

THE author of this pleasant little book is the current president of the British Psychoanalytical Society and a past president of the Pediatric Section of the Royal Society of Medicine. His material is presented in an informal conversational style and is addressed to "the ordinary, devoted mother," apparently a paragon of intuitive wisdom in whom the author has great faith. In no way does the author suggest that he is writing a book to be used by professionals, but the distribution of the book in the Basic Book series offers evidence that the publishers, at least, have such intentions. Perhaps it is accurate to describe the volume as aimed at the "professional laity" in the field of mental health and child care.

The word *relationships* in the subtitle is helpful in delimiting the area of emphasis of the book. It makes no attempt to provide a comprehensive coverage of every area of childhood problems, as in

Spock; nor is there an assemblage of data relevant to a particular point of view, as in Gesell. Rather there is concern only with the types of relationship which should characteristically be found between a mother and her child, and different aspects of child development are discussed only to highlight these types.

Basically Dr. Winnicott's approach to his "ordinary, devoted mother" is supportive, designed to bolster her confidence in her natural ability to deal with daily life problems. There is almost a subtle Coué-ishness in the following:

I want you to be able to feel confident about your capacity as mothers and not feel that because you could not know about vitamins you also could not know about, for instance, how to hold your infant. . . . You are not anxious and so are not gripping too tight. You are not afraid you will throw the baby on to the floor. You just adapt the pressure of your arms to the baby's needs, and you move slightly, and perhaps you make some sounds. The baby feels you breathing. There is warmth that comes from your breath and your skin, and the baby finds your holding to be good (pp. 98-99).

From the preface on the author insists that he is not going to try to tell mothers what to do but plans merely to write about what it all means. Of course, he does tell mothers what to do, implicitly and subtly; but in doing so he uses a technique important in advertising, viz., the advantages of a product are mentioned only in the form of a reminder of things the listener does not need to be reminded of for the good

reason that he already knows them. This is undoubtedly a good trick in both journalism and interpersonal relationships; yet it does seem a little hazardous to encourage the mother to rely entirely on intuition and to regard as suspect the idea that it might be possible to learn something about being a mother. Throughout the book the mother's intelligence and the need to learn anything about appropriate maternal behavior are minimized. For example, "If a child can play with a doll, you can be an ordinary devoted mother, and I believe you are this most of the time. Isn't it strange that such a tremendously important thing should depend so little on exceptional intelligence? . . . If you did it all intuitively, probably that was the best way."

The author shares with other current psychoanalytic writers a penchant for projecting himself into the mind (the word is used deliberately if not advisedly) and the feeling state of the infant. An outstanding example of this technic is the paper by Anna Freud in Vol. VIII of *The Psychoanalytic Study of the Child*. Perhaps such descriptions are no more presumptuous than the famous guess of William James that the mind of an infant was a "big, blooming, buzzing, confusion." Yet somehow that description seems more parsimonious than such a one as: "As you know, a baby sometimes cries when he is dirty. This might mean that the baby does not like being dirty (and, of course, if he remains dirty long enough his skin will become chafed and hurt him), but usually it means nothing of the kind—it means that he fears the disturbance he has learned to expect. Experience has shown him that the next few minutes will bring about a failure of all the reassurances; he will be uncovered and moved, and he will lose heat."

Or perhaps the following passage on weaning is an even better illustration: "The baby is sad at times like the weaning time because circumstances have made anger come and spoil something that was good. In the baby's dreams the breasts are no longer good; they have been hated and so now they are felt to be bad, even dangerous. That is why there is a place for the

wicked woman in the fairy stories who gives poisoned apples. For the newly weaned infant it is the really good mother whose breasts have become bad, and so there has to be time allowed for recovery and readjustment." Any description of the 'meaning' of an experience to an infant must be presumptive, and it seems only cricket to remind the reader of that fact occasionally.

IN a book purportedly written for nonprofessionals, there is always the problem of how to handle the presentation of evidence. Or, stated another way, what is the proper balance of facts and generalizations? It does not seem that the ideal solution is complete avoidance of evidence—especially in a field in which generalizations are difficult to make because of conflicting sets of 'facts.' On page 88 the author states a big generalization: "On the whole it is those who once freely enjoyed some pleasure or privilege who are best able to give it up if it becomes necessary." Since this statement appears in a chapter entitled *Weaning*, it would not be inappropriate to refer here to Sears and Wise's study of thumb-sucking and other oral habits. Certainly the findings of this latter study would not lead comfortably to the same generalization. Perhaps the author's "On the whole" covers the situation, but in many other instances the link between evidence and generalization should be stronger.

Dr. Winnicott gives a great deal of space to another concept which especially interested this reviewer. This idea is perhaps best expressed in the chapter entitled, *The Innate Morality of the Baby*. The resemblance to Rousseau's doctrine of innate goodness is more than coincidental. The counterpart of this aid to training is "innate guilt," which gradually emerges and permits the innate morality to become manifest. Apparently the negative poles of these attributes are *imposed* morality and *imposed* guilt, which the foolish parent can force upon the child by improperly timed and unduly harsh training methods. While various negative consequences of such training are implied, at least one is made explicit: "Imposed morality bores us." Again in

this section no attempt is made to offer data—even the education of an Émile—to bolster the generalization, but the firmness of the author's conviction stands out in the last two sentences of the chapter devoted to this topic: "Little children are fiercely moral. It is for you to catch on to their primitive morality and to tone it down gradually to the humanity that comes from mature understanding."

It seems that most of the points made by the reviewer have been negative.

Perhaps the "ordinary devoted mother," in her intuitive wisdom, will somehow know how to disregard those generalizations that lack sufficient data to lean on and will instead respond only to the warmth, sincerity, and general 'good feeling' Dr. Winnicott manages to convey by his style of writing. Nevertheless, the critical reader, whether an ordinary devoted mother or not, is likely to continue to feel a little bit uncomfortable all the way from the Preface to the Postscript.

Self-Statements in Personality Assessment

Allen L. Edwards

The Social Desirability Variable in Personality Assessment and Research.
New York: Dryden Press, 1957. Pp. xiv + 108. \$2.75.

Reviewed by JERRY S. WIGGINS

Dr. Wiggins is Assistant Professor of Psychology at Stanford University. He has a recent PhD from Indiana University and had a brief term of teaching at the University of Rochester. Now he is teaching undergraduates and graduates about the measurement and evaluation of personality. Inspired by Edwards he has been examining social desirability as it enters into the Minnesota Multiphasic Inventory—the "MMPI." He admits to some suspicions about social desirability in personality assessment plus a second-order suspicion that his first-order suspicion may be due to his personality. Psychologists always get into that kind of trouble.

HISTORICALLY those who have used personality inventories and other instruments of the self-report variety have been regarded with suspicion by those others who insist that there is a fatal flaw in this type of measurement. Certainly, it is not an overly cynical view of human nature that doubts the faultless veracity of subjects in a situation that requires the surrender of their most intimate thoughts and feelings to an impersonal IBM answer sheet. The myopic focus of American mental test-

ers on measured objective responses seems to have given rise to the notion that the most direct route to the assessment of a personality trait or the detection of a symptom lies in simply asking a subject whether or not he has the trait or symptom.

During the past twenty-five years, a considerable body of studies has accumulated to make visible the pitfalls of this line of reasoning. The demonstrations of the 'fakability' of most self-report measures, plus the agonizing reappraisals of the actual predictive validity of rationally-derived measures (e.g., Ellis, 1946), have done much toward taking the fun out of inventory assessment. It was the *empirical* approach to the problem of item content and truthfulness that provided both a method that worked and an attitude that lent respectability to the use of paper-and-pencil devices. Here the view of a self-rating, as Meehl (1945) put it, was: "an intrinsically interesting and significant bit of verbal behavior, the nontest correlates of which must be discovered by empirical means." This is probably only a temporary solution for the problem of accuracy of self-report, as is evident from the efforts of the

same empiricists to measure 'dissimulation' and 'lying' on inventory items. Nevertheless the armamentarium of 'validity' scales for detecting deception in report has permitted the growth of a certain smugness in users of these modern inventories.

Professor Allen Edwards of the University of Washington does much, in the little monograph under review, toward reopening the old wounds of the personality inventories and inflicting fresh ones on Q-sorts, adjective check lists, and the like. More than this, he attempts to recast the problem of self-report in a new mold that contains a basic reformulation and several suggested solutions. Many readers know Dr. Edwards for his texts on experimental design, statistics and attitude-scale construction (CP, 1957, 2, 237f.). Some may not know that he is a constructor of a personality test in his own right (*Edwards Personal Preference Schedule*). He is thus singularly qualified to present the topic of social desirability to us by virtue of being the originator and principal investigator of a problem which is currently assuming the status of an 'issue' in psychological journals. This monograph, which is an extensive revision and elaboration of his presidential address before the Western Psychological Association, carefully assembles the many results of five years' active experimentation centered around this issue.

When judges are instructed to rate the desirability or undesirability of traits implied by personality statements, they do so in terms of a continuum that Edwards feels is "the most important single dimension on which to locate personality statements." A social-desirability value may be obtained for any such item by application of one of the conventional psychological scaling methods. People tend to evaluate these items consistently, whether they be college students, Japanese-Americans, high or low status adolescents, Norwegians, psychiatric patients, or 'Skid-Row' alcoholic TB patients. More striking than cross-cultural psychophysics, however, is the use to which the scale values themselves can be put.

Armed with the values on a social-desirability scale for a group of person-

ality statements, one can account for about 76% of the variance involved when the same items are administered to subjects or patients in inventory, Q-sort or adjective check-list format. This is a dramatic way of saying that the correlation coefficient between the number of people who answer 'true' to an item in a personality inventory and the independently judged social-desirability scale value of that item is about .87. This finding has been replicated with such regularity that one has no alternative to accepting it as a fact.

It is the implications of this fact for personality assessment and research that



ALLEN L. EDWARDS

constitute the main body of this monograph. Edwards makes a pallid correlational result come to life and subsume such unquestionable techniques as the Q-sort and the mighty MMPI. One is tempted, at points, to view all self-report devices as simply crude measures of social desirability 'set' and even to view psychological illness as a kind of pathology of this set. Nor would this kind of expansive extrapolation be discouraged by the tone of the monograph. The weighty evidence, moreover, produces such a sense of urgency that one almost becomes receptive to any proposed solution of this dilemma. Yet there are many points in the fast moving argument at which some will drag their feet.

To what extent do these group correlations reflect individual tendencies to answer items in terms of their perceived social desirability? A study by Rosen,

which is discussed in the present monograph, may provide insight into this problem, but some would say that the amount of space devoted in the monograph to arguing away Rosen's findings might have been better employed in presenting a more complete picture of what he did.

What are the psychological components of the social desirability set? Distinctions between conscious and unconscious distortion, subtle and obvious items, faking 'good' and 'bad' are soft-pedaled here. We are told only that this set is all that is 'purely' measured by the K-scale of the MMPI as well as other unspecified things. At times, the trait becomes so pervasive that one wonders if he might anticipate individual differences in it. Certainly the concept would benefit from a sharper delineation of the defensive tendencies involved, and certainly the specific clinical and personality scales that are correlated with this tendency suggest something about it as well as revealing their own susceptibility to it. On the other hand, one wonders why an author who is so favorably disposed to 'operationism' chooses to ignore the metaphysically pure 'deviant response-set' interpretations of personality inventories that have been offered by Berg (1955) and Barnes (1956).

Does the forced-choice item format suggested by the author really control for this tendency? The evidence presented clearly supports the contention that this format (as exemplified in the *Personal Preference Schedule*) does much to control this insidious test-taking attitude. Evidence from other quarters is just beginning to appear in the psychological journals, however, and the final judgment may be close to a draw.

Much of the burden of proof for the assertions made appears to rest on studies that have employed a 'social desirability scale' (SD) which purports to measure the test-taking attitude under discussion. This scale, which appears to be essentially an anxiety scale with reverse keying, fails to satisfy the three criteria that the author emphasizes throughout the monograph: (1) the judges' ratings were not obtained under the standard instructions; (2) no attempt was made to scale the items in

the suggested manner; (3) the items do not appear to be heterogeneous or independent of the trait of 'anxiety.' This is the scale that led Edwards to the questionable position that subjects who obtain low scores on the Taylor Anxiety Scale are more 'anxious' than subjects who score high.

Despite the fact that many of the author's assertions are of relatively low 'social desirability value' in the present psychological scene, this little monograph seems likely to have an impact on current conceptions of the devices that depend on self-report. It is a *must* for all who are directly or peripher-

ally concerned with such tests. Some knowledge of MMPI and other scales is required to thread one's way meaningfully through the maze of intercorrelations presented. Some empathy with the problems of the constructor of paper-and-pencil tests is also required to appreciate a theoretical argument that 'predicts' signs of correlation coefficients. Nevertheless those whose research makes them vulnerable to the currently fashionable bit of seminarmanship: "But couldn't you account for your findings equally well in terms of social desirability?" should familiarize themselves with the full implications of the charge.

methodologists. The latter are regarded for the most part as obstructive, pedestrian thinkers who throw up roadblocks against the advancing modernists. Whether Ferster, Skinner, and the rest are in a defensive or offensive deployment may be argued, but *Schedules* surely represents a major *tour de force* for their group. Here are presented five years of laboratory effort plus three years of analysis and writing by an able, well-integrated team, totally convinced of the virtues of their method, and well supported with funds, equipment, technicians, and the like. The strengths and shortcomings of the book are the strengths and shortcomings of the 'Skinnerian' approach. Here they are displayed for all to see.

Pigeons Peck for Positivism

C. B. Ferster and B. F. Skinner

Schedules of Reinforcement. New York: Appleton-Century-Crofts, 1957.
Pp. 740. \$9.50.

Reviewed by DAVID A. GRANT

People think of Dr. Grant as a 'learning psychologist,' for his major research has lain in that field. He says he is an experimental psychologist and is "proud of it." People also think of him as a statistical psychologist because he is so competent in that field, but he is very sure that statistics is the servant of experimentation: use statistics to extract a theory from data, not to manipulate the data to yield an already selected theory. He is Professor of Psychology at the University of Wisconsin.

THE first author is Dr. Charles B. Ferster who received his indoctrination in the experimental analysis of behavior at Columbia University and has been a member of the Pigeon Staff at Harvard University. Professor B. F. Skinner, the second author, obtained his PhD at Harvard in 1931, presenting a thesis on the operant conditioning of rats. After developing a congenial relationship with pigeons elsewhere during and subsequent to World War II, he returned to Harvard as Professor of Psychology in 1948. His recent work has been devoted to exploring possible

applications of his techniques and conceptions to classroom learning, behavior pathologies, social behavior, and various kinds of human conduct.

Schedules of Reinforcement reports what Ferster and Skinner consider to be the important and representative findings of a program of research that was "designed to evaluate the extent to which an organism's own behavior enters into the determination of its subsequent behavior" (p. 3). These results are described in 741 quarto pages which include 921 figures. Over 900 of the figures present primary data, the pecking of pigeons, in the conventional cumulative-frequency graphs. These numbers are cited to show the magnitude of the task remaining for the reader (or reviewer) of *Schedules* after Ferster and Skinner had done with their "analysis."

An appreciation of this book requires that it be put into its strategic context. For some years Skinner and his adherents have passionately, often eloquently, declaimed the virtues of "the modern study of reinforcement" and exposed the limitations of "traditional" theoretical-experimental psychologists and

SCHEDULES must be compared with Skinner's 1938 *The Behavior of Organisms* in order to see what developments have come about in twenty years. The first glance shows that although techniques of scheduling contingencies of response and reinforcers have developed greatly, recording and analysis of data remain unchanged. A certain evangelical tone, present in *Behavior*, is absent in *Schedules*. (This, however, is discrimination, not extinction.) All references in *Schedules* are to Skinner, Ferster, and members of their group; *Behavior* was far more catholic in citation if not in essence. This, and the total absence of explanatory concepts such as the "reflex reserve" in *Schedules*, presumably represent extinction. (An amusing example of extinction within *Schedules*: the occasional summary sections are emitted at slower rates until they disappear entirely after page 203.) *Schedules* extends the ratios and intervals of *Behavior's* fixed ratio and "periodic" reinforcement. Ingenious instrumentation permits exploration in *Schedules* of variable ratios, variable intervals, differential reinforcement of rate of responding and a variety of intermixtures of the foregoing conditions. Insertion of 'time-outs' (lights off—no pecking) into the schedules reveal more clearly some effects of stimuli occurring at critical times than was possible in 1938. Some major topics of *Behavior*, such as Drive, are scarcely dealt with

at all in *Schedules*. But *Schedules* reports the effects of time-correlated visual stimuli (clocks) and response-correlated visual stimuli (counters) which were lacking in 1938.

Generally speaking, the thesis that response rates, temporal patterns of rates, and patterning of rate in the temporal vicinity of the reinforcer are dependent upon the schedule of reinforcement is demonstrated clearly enough. But this was clear in 1938. Faster constant rates (from *variable ratios*), slower rates in well-starved birds (from differential reinforcement of slow responding), oscillation of rate (from low *fixed intervals*), and more complicated shifts in rates and very high sustained rates (from complicated schedules) are new. Although in one sense they represent fairly obvious extensions of the 1938 findings, in another sense the schedules are used more analytically to disentangle some of the complex contingencies of reinforcement. For example, the *variable interval* schedule is claimed to produce constant rates, because no single feature of the elapsed time or response rate acquires discriminative properties. Perhaps the most dramatic observations are the effects of the clocks and counters on response rate. (The essential data reported in *Schedules* on the effects of clocks and counters have been given earlier by Skinner in various public lectures.)

It is interesting to see how the tenets of Skinner's methodology affect the achievement of the authors' intent to enable one "to predict the effect of any schedule" (3). The critical event in Skinner's psychology is the contingency of behavior and reinforcer or reward. Because the contingencies are unique in any single sequence or schedule of behavior and reinforcers, Skinner has long held that reduction of data by averaging or other statistical device yields meaningless results. (Nor, for that matter, can meaningful control experiments be run, although Ferster and Skinner, paradoxically, express regret at the absence of a performance without a counter to compare with behavior on a mixed schedule with a counter on page 606. Perhaps some things are more

unique than others.) In consequence, no detailed quantitative laws emerge from 70,000 hours of data-gathering. Interpolation, to say nothing of extrapolation, is not really possible. Brief consideration of the inexorable arithmetic of permutations and combinations then reveals that the 900 figures cannot permit detailed prediction of "the effect of any schedule." Actually a worker can get some idea of what is likely to occur with most of the schedules he is tempted to use. But usually the two (or, rarely, four) birds reported on a given schedule give somewhat differing, and occasionally widely differing, results. Also, these birds will very likely have had complicated, and usually incompletely reported, past schedules. What then? Well, the worker is put in the position of navigating coastal waters with a *Rand-McNally* road map as his guide. It can be done by an experienced man, but even he would be safer with a *Coastal Pilot*.

From the standpoint of the efficient communication of scientific ideas, *Schedules* is a mighty noisy device. The reviewer cannot believe that all 250 million of those pecks are signal. Since statistical summarizing is disdained, the alternatives are to report all data or to select representative examples. Ferster and Skinner are forced to do the latter. Assuming that they passed the tests of genius and character prerequisite to presenting the 'right' 5-10% of their findings, the authors must then leave to their readers the frustrating task of attempting any summarizing. Clearly, Ferster and Skinner believe it cannot properly be done, so that the culmination of all their program is indeed a bulky, cumbersome atlas and a crude one at that.

The defects of *Schedules* seem to this reviewer to arise more from the style of science advocated by Skinner and enthusiastically accepted by Ferster and other followers rather than from the complexities of the behavior or inadequacies of operant training. The importance of the behavior-reinforcer contingency has long been accepted by psychologists, and Skinner's students have utilized the basic conception in both direct and subtle ways to achieve a superb technique of animal training. *Schedules*

gives tantalizing glimpses of the power of the operant as a tool to investigate psychopharmacological and neurophysiological problems. To patient investigators of the future, Ferster and Skinner leave the task of exploitation. These future workers will inevitably have to work in a more orderly and systematic fashion if they are to achieve their scientific and technological goals. Their departures from Skinner's positivistic haven will have to be more deliberate and sophisticated—less 'artistic,' less idiosyncratic.

A Text on Suicide

Edwin S. Shneidman and Norman L. Farberow (Eds.)

Clues to Suicide. New York: Blakiston Division, McGraw-Hill, 1957. Pp. xii + 227. \$5.50.

Reviewed by RAY M. SCHUMACHER

who began his professional life as an engineer, is now—largely as a result of his interest in 'students and people—a clinical psychologist, a Staff Member of the Counseling Center of the San José State College, where he is also Assistant Professor of Psychology. In addition he is Clinical Psychologist for the Community Mental Health Services of San José, California.

If the reviewer had not been aware of the need for more information on the subject of suicide, it would have been made clear to him by the great interest shown by members of various professions who happened to observe his copy of this book. Both theoretical and practical aspects of this problem warrant more extensive investigation, and it is to this need that Shneidman and Farberow have addressed themselves. Dr. Shneidman is Chief for Research in the Psychology Service of the Veterans Administration's Neuropsychiatric Hospital in Los Angeles, and he is also connected with the University of Southern California. Dr. Farberow is a Clinical Psychologist in the Veterans Ad-

ministration's Mental Hygiene Clinic in Los Angeles, and he teaches at the University of California at Los Angeles. Both editors are Diplomates in Clinical Psychology and Fellows in the American Psychological Association's Division of Clinical Psychology.

Although the editors state that the book is directed primarily to the practitioner, the essays are divided into sections emphasizing theoretical and experimental problems, on the one hand, and clinical considerations, on the other. In each section the editors attempt to include articles dealing with a variety of the aspects of suicide. This range of concern is simultaneously a strength and a weakness of the book. Although this mode of approach makes available considerable information and provides an array of theories and hypotheses, the reader who seeks an integration of the information in this area and a clarification of the basic issues involved is likely to be disappointed. As Jackson points out in the second chapter, suicide is best considered as a symptomatic act, not a discrete event. A study of suicide as such would then be expected to involve a wide diversity of subjects often bearing such slight relationship to one another that difficulties in organization should be expected. Nevertheless, it would seem that articles might have been selected in such a manner that a clearer organization would have been introduced in the book. There might also have been some sort of summary chapter which sought to integrate the various contributions.

SOME criticisms may be mentioned which apply to a number of the included articles. The psychoanalytic concept of 'hostility turned inward' would seem to be too readily accepted as a basic dynamic of depression and suicide. No verification is offered for this view, yet some of the authors appear to use this concept as a basis for further theorizing without clear awareness that this concept itself is a theory that is not yet scientifically established. Although it is doubtful that suicide can be usefully considered as a discrete entity, some authors attempt to apply a single theory or concept to various types of sui-

cides under various conditions. A noteworthy exception is the article by the editors which investigates the relationship between age and motive for suicide as indicated by the content of suicide notes. The fact that some significant differences between age groups were found urges caution in the generalizing with respect to a single motive or set of motives.

Although a number of articles dealt with philosophical facets of the suicide problem, only the chapter by Silving raised the question of the 'right to dispose of one's own life.' Related to this, perhaps, is the attitude of some of the articles which generally assumed that suicide is inevitably an irrational act. Both of these points may be very much involved in the philosophy and techniques of the prevention of suicide and therefore crucial to one of the important concerns of this book. A less biased account would have resulted if there had been included views and material covering both sides of these somewhat controversial questions.

Considerable emphasis is placed on the detection of potential suicides and the fact that indications of the danger of suicide are often available but not utilized. The importance of these 'false negatives' is, of course, great, but the implications and importance of 'false positives' were largely ignored in this book. On the other hand, too hasty an acceptance of the likelihood of the patient's suicide by the relatives and especially by the therapist (when the person is in treatment) may have serious consequences for the individual's self-esteem. Clues to suicide are important but clues to nonsuicide are also vital to the practitioner who must sometimes take a calculated risk. The very useful therapeutic and preventative measures described in the latter part of this book could be actually harmful if inappropriately applied.

SOME contributions merit special mention. The comprehensive study conducted by the editors, providing the material for a number of the articles, is interesting and valuable. Their sample is large and generally free from selective bias, and the controls are quite ade-

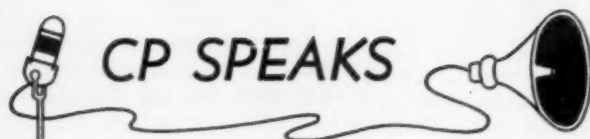
quate. The publication of the full investigation may well be a major contribution.

It should be said that the study of suicide notes, although very interesting and provocative, must be interpreted with extreme caution. Although the authors point out that the 15 per cent of all suicides during the period covered who left notes seem no different from the total sample with respect to sociological, economic, and other available data, they are nevertheless obviously different with respect to the personality dynamics that determine whether or not a person writes a note at this dramatic point in his life. It may well be that results from the study of such notes are not applicable to suicides in general.

The sociological article by Henry and Short offered an interesting explanation of the correlation of frequency of suicide with higher social status and with lower strength of the individual's relational system. Although their concept of "freedom from external restraint" seems to fit the described data, alternative interpretations of these correlations are certainly possible. For example, the view that higher status implies less "conformance of behavior to the demands and expectations of others" appears to be an oversimplification and deserving of further study.

On the practical side, the article by Litman stands out. It is authoritative, candid, and specific. Other therapists may not approve all of his techniques, but the techniques are clearly described and clearly related to the specific problem of treatment of the potentially suicidal patient.

All in all, the articles are absorbing and useful. If the total effort falls short of the hope for integration in this important area, nevertheless it may be said that this fault is partially inherent in the complexities of the problem and the lack of sufficient previous study. The editors hope that their book will have both practical and heuristic values. For the reviewer, it had both—and in quantity. The volume teaches a great deal and stimulates further reading and investigation.



CP SPEAKS

WHO READS CP?

WHO talks to whom about what in CP? That was the question that occupied this page some months ago (CP, Feb. 1958, 3, 33). CP knew about the character of its reviewers, somewhat less about the authors of its books, and very little about its readers. Now it knows about the readers, for a loyal friend, who must have in her ancestry both a calculating machine and a dictionary, has analyzed CP's subscription tapes as they existed back in November 1957, the tapes for subscriptions other than for members of the American Psychological Association. There were then about 4700 member-subscriptions, and these data show that they constituted about 85 per cent of the total readers. They are scattered, of course, through the wide range of the APA and its 18 divisions, and that is a wide spread. (CP tells its reviewers: Teach! Talk to the readers about the book as you would to bright sophomores who never had a course in the subject.) The other 15 per cent of the list provides a measure of CP's influence outside of the home base. Who is there, not in the APA, that buys and may read CP? The table shows the frequencies.

	U.S.A.	Foreign	Total
Individuals, not APA	82	30	112
Colleges	300	80	380
Medical institutions	103	11	114
Public libraries	11	10	21
Governmental institutions	36	18	54
Publishers, booksellers	11	10	21
Businesses	25	3	28
Miscellaneous	33	25	58
Total	601	187	788

The 187 foreign subscriptions are distributed in this way: England, 22; Canada, 21; Japan, 17; Australia, 12; Sweden, 12; Netherlands, 10; France, 9; South Africa, 9; Italy, 7; 6 other countries, 4-6 each; 12 countries, 2-3 each; 13 countries, 1 each.

The 380 Colleges include 252 United States colleges and universities (and 59 foreign); 20 U. S. Departments of Psychology (and 13 foreign), 22 U. S. Schools of Education (and 8 foreign), and 6 seminars. There are 70 U. S. medical institutions (plus 11 foreign) and 33 hospitals of the U. S. Veterans' Administration. Of the governmental institutions 45 are national (27 U. S., 18 foreign) and 9 are local governmental bodies. *Miscellaneous* includes 8 counseling and guidance centers, 5 penal institutions, 4 psychoanalytic institutes, and all sorts of cultural bodies athirst for wisdom, like Care, Inc., The Rockefeller Foundation, and the Ford Foundation.

So there you are, Reviewer! When you write a review for CP, you are addressing, let us say (as of more than a year ago when these data were obtained), a large group of people interested in psychology and its books, very heterogeneous indeed as to special interest and knowledge, but consisting approximately of

4700 United States residents, members of the APA,
600 United States residents, not APA members,
100 residents of other English-speaking countries,
100 residents of non-English-speaking countries.

That does not make CP an international journal. The new *International Directory of Psychologists* has about

7000 entries and the APA has about 17,000 members. To the American reviewer, CP says: Don't forget the foreigner; psychology needs to keep pushing toward internationalism; wisdom spreads broad; be as xenophilic as you can. To the foreign reviewer, CP says: Remember, you are writing mostly to Americans, the modern positivistic American behaviorist psychologist, who means, even when he fails, to stick close to empirical data, if possible to experimental data, and thinks that psychology is not a conversational science; it is your responsibility to make him want to read what you say and to understand you when he does.

REASSURING CRITICISM

SOMETIMES even neurotic CP gets reassured by criticism. It happens when CP has been worrying over a No. 1 criticism (might it be right? might the critic really be talking sense?) and then a No. 2 criticism comes in and exactly cancels No. 1. Then it is that CP gets right back on the donkey and, head high, rides it across the bridge. Of course a journal of criticism ought to be criticized. Criticism is a phenomenon. It is interesting. It tells you, for one thing, something about the critic.

This time CP was worrying over its coverage. It is so sure that it ought not to be reviewing the books that the Editor or his Consultants throw out as not worth reviewing. CP's critics never name particular books that CP misses, but they worry because the list of *Books Received* is so much larger than the number of books actually reviewed (CP, Oct. 1957, 2, 261). CP is against censorship. Is this autocratic denial of CP's forum to some who are pleading for an audience, is it consistent with the First Amendment to the Constitution of the United States? And then blithely the No. 2 critic walks into CP's office. "Why in the world does CP review so many trivial books? CP would be a better magazine if it did not stoop so low." Well, No. 2 answers No. 1, and conversely. CP will go a little way toward making a few final negative value judgments without having the expert give the book a working over, but, beyond that degree of unavoidable polic-

ing, every book that is current and can count as psychology has the right to the judgment of its peers, and the peers are CP's readers. Even the negative review is not final. One of CP's readers said the other day that he rather liked the sound of a book that CP's reviewer had cudgelled. The reader remarked that he, knowing the reviewer had been brought up in Europe where the academics are accustomed to play rough, had ignored the reviewer's value judgments and had had his interest caught by the description of content. You just do not know that a second-rate book is second-rate until it has been reviewed, and certainly not always then.

VOLUNTARY REVIEWS

FOR the most part CP publishes reviews that it has invited the reviewer to write. The Consultants chose the reviewers for CP, and that tends to limit the reviewing to the 'experts' whom the twenty-seven Consultants know and who are willing to accept the invitation when it is made—over two hundred different reviewers in a year. How are other reviewers, who are competent and willing, to be included among CP's contributors? How can the range of the Consultants' professional acquaintance be extended?

If a person writes to CP offering his services as a reviewer in a particular field, CP sends the letter or the data in it to the Consultant in that field, or the data to the Consultants in several overlapping fields. The letter needs, however, to express more than willingness. It needs to show competence, and it is hard to attest your own competence, although some of these willing reviewers do this job very well, sustaining an aura of objectivity all around their self-appraisals. The competence wanted is, of course, not merely competence in the field, but also the competence to write interestingly—for CP demands more than correctness and clarity.

Quite a number of persons write to CP asking to review a particular book, and once in a while a would-be reviewer sends in the MS of a review. Either these persons have seen the book or have seen it listed in CP's BOOKS RECEIVED. Except for an occasional European book, CP can rarely accept this

offer, primarily for the reason that the book in question is already taken care of. The review of it may already be in galley proof, or at least the reviewer has been selected and is at work with his deadline approaching. By the time BOOKS RECEIVED is published it is almost always too late to ask to review any book in the list, for by then it will be at least two months since the book came in, and CP is not so slow in getting a review under way.

If you are going to advise CP what to do about a book—and CP is avid for other people's wisdom—you have got to do it within ten days of the publication of the book. And the same goes for special advice, like the book's being good enough for a double review; CP always gets this advice too late. The ideal time to write CP about what it should do with a book is just before the book is published! Sometimes that does happen, and then CP is delighted to have its *Einstellungen* all properly set up beforehand.

There is, however, another way in which a frustrated willing reviewer can reduce his tension. He can write a letter to ON THE OTHER HAND. If he dissents from the review as published, he can make his letter into a counter-review. If he thinks the book should have had a double review when it did not, he can add the missing half in his letter. He can augment or depreciate, praise or dispraise. He can have his say, provided he is interesting and sufficiently impersonal. See CP's comment on cybernetic justice, a comment about how this kind of verbal jousting ought to work out (CP, Aug. 1957, 2, 210f.). ON THE OTHER HAND could turn into an exciting intellectual tournament if CP's readers could but start breaking their verbal lances under CP's simple and benign rules of chivalry.

—E. G. B.



To the question whether I am a pessimist or an optimist, I answer that my knowledge is pessimistic, but my willing and hoping are optimistic.

—ALBERT SCHWEITZER

Love for Hobos

Alexander Vexliard

Le clochard: étude de psychologie sociale. Bruges: Desclée de Brouwer, 1957. Pp. 317. 200 fb.

Reviewed by NICHOLAS HOBBS

who is Professor of Psychology at George Peabody College in Nashville. His chief concerns are human development, mental health, psychotherapy, parent-child relations, and, at least for the moment, unloved hobos. So far he has written 17 reviews per annum for CP, too many to cite—Harry Stack Sullivan, Miner, Symonds, Prescott.

THE literature of the vagrant is surprisingly extensive to one come new to it. Perhaps so much has been written about the vagrant because he is a social problem, perhaps because he provides an opportunity for social commentary, as in the work of John Steinbeck, and possibly because he has a certain charm and fascination for us all. The great hobo clown, Emmett Kelly, the several flamboyant hobo 'kings,' Picasso's wandering minstrels, Robert Frost's tramps, all have tremendous appeal. Do they permit us vicarious enjoyment of irresponsibility? Perhaps, but this is not all, for they evoke sympathy too. And what a fine vigorous vocabulary they have spawned: *bum*, *bindle-stiff*, *rodman*, *weary Willie*, *wet-back*, *skid-row*, *jungle*, and then *hobo* itself. According to this French source, the word *hobo* is an Americanism denoting a wandering rural worker, "hoe boy"; but Webster's New World Dictionary returns the compliment, maintaining that the word is a French greeting of sorts, "Ho, Beau!" Neither explanation is convincing. The French have done better semantically in *le clochard*, "the limping one," from the verb *clocher*, "to limp," which also means to be defective. (Cf. *raisonnement qui cloche*, lame argument.) It is interesting that our society has about as many discriminating words for vagrant as the Eskimo has for snow.

Vexliard, a social psychologist with

strong leanings toward the clinical, here reports a study of 61 vagrants of Paris, unselected except by such sundry determinants as operated to bring the author and his subjects together. The chief instrument of appraisal was the clinical interview, summaries of which he presents for all his cases. He used tests too: the Stanford-Binet, Raven's Matrices, some psychomotor tests, the Rorschach, the T.A.T., the Rosenzweig. The integration of all this material is accomplished without statistical ordering; in the European tradition, Vexliard is the synthesizer, working with raw data and moving readily to conclusions, using intuitive confidence levels. American psychologists will regard his findings as enlightened hypotheses still needing testing.

VEXLIARD maintains that the legalistic approach to vagrancy is unrealistic and only pushes the vagrant toward delinquency. He rejects the popular notion that the vagrant is a schizophrenic or a psychopath, maintaining that such disturbances are exceptional. His major thesis is that the hobo is not psychotic or neurotic but simply *beat*. The hobo is often physically ill, sometimes crippled, always disheartened, usually detached from people. "The essential thing for him is not to get engaged." He has experienced so little positive reinforcement in life that he settles for a tentative and often dubious commerce with the world, simply that he may survive.

The author believes, without substantiating data on this important point, that most vagrants will respond to rehabilitation programs involving environmental manipulation or social therapy and that those few who are unresponsive to such treatment, often regarded as incurable, will require and can respond to treatment focusing on values and making use of emulation. Rehabilitation requires an appropriate association with a person from whom the hobo can gain new values. The hobo has no one to love. The therapist must be able to give him the most precious of capacities, the ability to love another and devote oneself to him. Individual therapy is, however, seldom to be recommended. Rather should rehabilitation proceed on a group

basis in "apprenticeship centers for adults" (some five or ten communities with from 300 to 500 vagrants are recommended for France), where routines of normal living can be established and where self-expression and initiative are

encouraged, surrounded with ambient hope, confidence, and love.

Vexliard's writing is simple and clear and, in the clinical sketches, often moving. His 391-item bibliography is marred by numerous errors in citations.

Perception's Straits of Messina

H. J. Eysenck, G. W. Granger, and J. C. Brengelmann

Perceptual Processes and Mental Illness. New York: Basic Books, 1957. Pp. 144. \$3.75.

Reviewed by JACQUELINE J. GOODNOW

Dr. Goodnow entered psychology via factor analysis at the University of Sydney in Australia, came to the United States and obtained a doctorate in clinical psychology at Harvard, spent two years in Germany on research in the classification and selection of personnel for the U.S. Army, returned to the States as a Lecturer on Psychology at Harvard, and is now working as a psychologist in the Division of Neuropsychiatry in the Walter Reed Army Institute of Research in Washington. All along she has been concerned with attention, discrimination, problem-solving, concept-formation, and their kind. With Bruner and Austin she is author of A Study of Thinking (CP, Oct. 1957, 2, 249-252).

EYSENCK in 1952 described the study of personality as "a dangerous course, threatened by the Scylla of nebulous . . . generalization, and by the Charybdis of pointlessly amassing unrelated 'facts.'" The present monograph from London University's Institute of Psychiatry starts off on a straightforward course but soon drifts into the whirlpool of Charybdis.

The danger of drift is present from the beginning, when the authors set out to explore how well some 80-odd tests differentiate among three groups of subjects: 106 normals, 20 neurotics, and 20 psychotics. It becomes more imminent as the three authors divide the tests among themselves, describe them in separate chapters and diverge in the

use to which they put the test scores. At times their only meeting-ground seems to be the common set of subjects.

Brengelmann searches for a way of testing some hypotheses about complex perceptual performance, drawn from German typologies of personality. Granger takes on the looser but still feasible goal of finding the richest kingdom, that test of visual function which yields the sharpest differences among the subjects. Eysenck looks all the way to Ithaca: support for the argument that perceptual tests might classify people more correctly than, say, the usual questionnaire, and, indirectly, support for his view that personality is hereditarily determined.

As long as we are in Brengelmann's hands, there is no problem. The questions he asks are novel and direct. Can we use "normal" and "abnormal" groups to test hypotheses about the perceptual performance of different personality types? Will the differences between "normal" and "abnormal" be the same as those ascribed to, say, the "integrated" and "disintegrated" types described by E. R. and W. Jaensch? The answer is affirmative, the evidence for it ample, and each test is informatively introduced by a reference to the way different personality types are expected to perform on it.

Most of the remaining tests are also introduced by a reference to previous work with them. It is here, however, that we enter Charybdis. For this previous work, most of it by investigators

associated with Eysenck, was already a direct comparison of normal and abnormal groups. The repetition of it runs directly counter to the warning that Granger himself gave in his excellent 1953 review: namely, that repeated use of a test should lead somewhere.

As Granger put it in 1953, repetition should at least draw together the results of several uses of the same test. Ideally, it should move on from being "purely descriptive and classificatory" and "be supplemented as early as possible by studies designed to determine the functions and mechanisms underlying the phenomena."

Granger's warning apparently went unheeded. The research is again classificatory. And, most unhappily, it does not build upon the results of previous go-rounds with the same tests. This is, for example, at least the fourth time that Eysenck or his associates have used a test of dark adaptation to discriminate between normal and abnormal groups. There is still no attempt to determine why it should have given such contradictory results. Body sway is an even more drastic case. It was voted "best-test" in Eysenck's *Dimensions of Personality* (1947), and was still highly regarded but barely significant in the *Scientific Study of Personality* (1952). Its current performance cannot even be compared with these previous ones. From the data given, the test seems to have discriminated poorly, but no measure of significance was computed. There is only the note: "variances nonhomogeneous" (p. 114). This is true, however, of many of the tests for which an analysis of variance was computed, with reservations but apparently no knowledge that nonparametric tests are available and would have avoided the impasse (p. 17).

IF we ignore the lack of useful ties with previous work, what is there to gain from the monograph standing by itself? Already mentioned is Brengelmann's chapter. In addition, the monograph allows a comparison of many different tests all given to the same population, and the presentation of results test-by-test is, moreover, an advantage for the readers who wish to shop around or to follow up a particular test.

Again, however, the lack of adequate direction and organization is a major problem. There are, for example, three tests on which time scores yield the rare and important result of clear differences between the neurotics and psychotics (pp. 43, 101, 113). These three tests are never brought together, and to only one of them is applied an imaginative and successful use of the concept of "blocking."

What direction there is, is imposed

by the use of factor analysis. Unfortunately, it is not a particularly appropriate technique for answering the questions Eysenck puts to it about the best way to sort out neurotics and psychotics from normals. For such a purpose, to complete Eysenck's metaphor, factor analysis is that half-helpful fig tree on the rock by Charybdis, to which Odysseus clung but which he had to abandon before his ultimately worthwhile journey could be continued.

The Folklore of the Survey Research Center

Robert L. Kahn and Charles F. Cannell

The Dynamics of Interviewing: Theory, Technique, and Cases. New York: John Wiley & Sons, 1957. Pp. x + 368. \$7.75.

Reviewed by BURTON R. FISHER

Dr. Fisher, with an eleven-year-old PhD from Yale, has been since 1951 Professor of Sociology at the University of Wisconsin. He has a right to speak confidently of the Survey Research Center at Michigan for he worked there in 1949-51 as Program Director of the Public Affairs Program.

POPULATION sampling, interviewing and content analysis of interview responses are three sets of tools that underlie survey research. Since their organization as a group within the U. S. Department of Agriculture before World War II, and thereafter as the Survey Research Center of the University of Michigan, Rensis Likert and his associates have exploited and in various degrees contributed to the development of each of these tools of research in social science.

I would venture the opinion that the principal published contributions of the Survey Research Center have been substantive, in its reports of survey findings. On the whole, its methodological publications have been fewer and less obviously impressive. Among them solid contributions have been made in the area of sampling. Little research has

been reported on the Center's work in content analysis.

The Dynamics of Interviewing is the first full hard-cover volume on a single major aspect of methodology to be written by members of the Center's staff. Its scope covers but extends well beyond the role of interviewing as a tool of surveys. Primarily an account of techniques developed and presented within a theoretical context, this volume is not, nor was it intended to be, a report of new, hypothesis-based research on interviewing. The title and provenance of the book might lead one to expect the latter (an expectation which is confirmed in Hyman's *Interviewing in Social Research*).

Kahn directs the Center's program on human relations in large organizations. Cannell heads this large organization's field staff. The book is an expansion of their 50-page chapter in *Research Methods in the Behavioral Sciences* (Festinger and Katz, Dryden Press, 1953). It makes apparent that they are connoisseurs of interviewing and rational consumers at the super-market of sophisticated psychological theory. They interweave and then apply to the interviewer-respondent situation

some leading current ideas (principally those identified with Lewin and Rogers) on perception, on motivation, on the psychological field, on role, on communication, on interaction processes in small groups and on the psychotherapeutic relationship. Their purpose is to give the interviewer an understanding of the psychodynamics of the two-person interview situation. Whether he be lawyer, physician, personnel man, journalist, or member of a survey's field staff, only with this understanding can the interviewer insightfully learn and follow the techniques the authors recommend. These techniques will elicit data from the respondent that meet the authors' explicit criteria of adequacy. Such are Kahn and Cannell's beliefs.

Despite the stress they put on comprehension of the underlying forces at work in an interview, the authors point out that the "information-getting interview," whose theory and techniques they are describing, has limited aims. It does not attempt to probe the depths of personality, or to evoke material from the unconscious, or to change the respondent. Its goals are thus only part of the set of goals of the physician or psychotherapist. In view of these limitations on the purposes of the "information-getting interview," it seems slightly contradictory to be told also that under "information" the authors include "values, feelings, hopes, plans and descriptions of self," even "partially formed attitudes" and "private, seldom-verbalized feelings," as well as "more objective factual data" in specific content areas.

To me, the most important point about the book is this: Kahn and Cannell have excellently and fully displayed, elucidated, and illustrated the Survey Research Center's armamentarium in questionnaire design and interview method. In this sense, the book is a very welcome 'first.' It will be particularly useful not only because it rehearses a wealth of *procedural* ideas and experiences, but also because it lays out the Center's *rationale* for "open-end" (roughly, "free-answer") questions and "controlled nondirective" follow-up of questions ("probes," à la Rogers). These are the hallmarks of the Center's inter-



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viewing operations. Throughout, the accent is on the interviewer's flexible art in striving toward data which are valid for the individual respondent; the dilemma is that standardization of behavior among interviewers may fall by the wayside in the drive for validity in each single case.

There is a helpful chapter on methods of acquiring interviewing skills, and a set of fully recorded interviews (social work, medical, supervisory, etc.). The authors suggest ways of using the latter in self-training, set the stage for each protocol, and act as Greek chorus in its unfolding.

The heart of the problem this book poses for me lies in its use of theory. The theory itself is persuasive—in part because it is so familiar, in part because it is so skillfully applied to the subject matter. It was included *precisely* because Kahn and Cannell believed that theory could contribute in training interviewers to obtain the respondent's cooperation and to elicit more responsive, full, unbiased, accurate, and relevant reactions from him. But even

without challenging the substance of the theory or the unique 'fit' of the theory and the techniques presented, I find myself asking some questions of a very pragmatic sort. Does the elaborate theory *actually* do for the interviewer what the authors believe it does? At minimum, what is the evidence that, by indoctrinating interviewers in *this* theoretical amalgam while they practice *these* techniques "to focus and control interaction," we do in fact get more adequate data? The authors label the "does and don'ts" approach to techniques as "folklore" and say that theory tells us "why" certain techniques work and when they are appropriate. Yet I cannot escape the feeling that the incantation of theory-words does not transmute "folklore" into science, or even provide a firm empirical base for the practice of an art.

I confess a strong liking for much of this Survey Research Center "folklore," so I suppose that what really bothers me is a hunch that so large and deep a theoretical substructure might in fact be dysfunctional. As I read the book and

thought about using it, I found myself wondering about the possibilities of effects on its audience—effects that I knew the authors never intended to produce. What concept of his role will the “information-getter” construct from the frequent ‘clinical’ references and from constant inferences of the respondent’s complex motivational structure? These references and inferences were utilized in the exposition of the theory. Will the interviewer confuse those unexpressed psychological forces, which the authors say he needs to recognize in the interview interaction, with the ‘depth’ of the

material to be sought and expressed in the interview?

Furthermore, the examples which the authors constructed to illustrate the theory’s application contain not only more material, but also more genotypical material than the ordinary interviewer is likely to derive from his short-time contacts with respondents. A lot of ‘clinical insight’ implicitly is assumed. What will this signify to the interviewer?

In sum, *would less stress on less theory better achieve the book’s own goals?* Perhaps I am conjuring up ghosts—but I should like to *know*.

scientist are structured by the conceptual apparatus furnished him by his mother tongue.

TÖNNIES used his typology in various ways. In the first place, responding to the evolutionary interests of his time, he described the course of social development in terms of an increase of the *Gesellschaft* features and a decrease—but not complete replacement—of the *Gemeinschaft* features of social life. Secondly, he used his typology as a tool in the analysis of the total array of social groups and relations which constitute the subject-matter of sociology. The task of this analysis was to ascertain the extent to which a given group or relationship approximates the *Gemeinschaft* or *Gesellschaft* type. Thirdly, Tönnies undertook a critical appraisal of the urban way of life, bringing to bear on this task not only his analytical scheme but also his anti-*Gesellschaft* value-judgments.

In the treatment of his problems, Tönnies benefited from the earlier literature in several respects. His notions of *Gemeinschaft* and *Gesellschaft* were partly derived from Sir Henry Maine’s discussion of *status* and *contract*, while Wilhelm Wundt’s distinction between two types of human will was adopted as a psychological underpinning. Hobbes’ *homo homini lupus* greatly impressed Tönnies, but what is for Hobbes a state of nature became for Tönnies a characteristic of *Gesellschaft* as an artifact. Marx furnished another component of the *Gesellschaft* construct, which is especially noticeable in Tönnies’ views regarding the role of labor in the *Gesellschaft* economy.

TÖNNIES’ own contributions to the work of later writers were also of considerable magnitude. Although he himself was not a prophet of catastrophe, his unsympathetic portrayal of *Gesellschaft* has furnished arguments to cultural pessimists from Spengler to Sorokin. Furthermore, his analysis was a source of ingredients for conceptual schemes which have been influential in modern sociology, especially those of Max Weber and Talcott Parsons. Through Redfield’s re-

A Sociological Classic Re-examined

Ferdinand Tönnies

Community and Society. (Ed. and trans. by Charles P. Loomis.) East Lansing: Michigan State University Press, 1957. Pp. xii + 298. \$7.50.

Reviewed by WERNER S. LANDECKER

Dr. Landecker is Associate Professor of Sociology at the University of Michigan. He received his training at the Universities of Berlin and Michigan, and holds doctors degrees—different kinds—from each institution. He is especially interested in the problems of social integration and stratification.

THE German original, which has been translated competently in this book, was first published in 1887. In this work, Ferdinand Tönnies (1855–1936) made a leading contribution to the development of sociology in Germany. Although subsequently he published many other theoretical analyses and empirical studies, he is still best known as the author of *Gemeinschaft und Gesellschaft*.

The chief purpose of this book, as stated later by Tönnies, was to provide a synthesis of two contrasting views in the history of social philosophy. The individualistic thinkers of the Enlightenment had seen human association as a contrived arrangement among rationally acting beings who strive for personal advantages, that is, as *Gesellschaft* in Tönnies’ sense. On the other hand, the

holistic thinkers of the Romantic period had viewed human association as *Gemeinschaft*, that is, as an organic system whose members are bound together by basic consensus and mutual affirmation. Tönnies’ intended synthesis was to be attained by the incorporation of both views into a more comprehensive scheme. He was so much biased in favor of *Gemeinschaft*, however, that its attempted synthesis with *Gesellschaft* reads more nearly like a comparison between the sacred and the profane.

What Tönnies did accomplish was the transformation of two common words of the German language into a typology which gave German sociology one of its major themes. In some respects this typology is similar to those constructed afterwards by Durkheim in France and by Cooley in the United States. Yet Tönnies’ typology has distinctive features, which reflect—at least in part—the special meaning which the words employed by Tönnies have in popular usage. This is one of several familiar instances where a comparison of social science materials in different languages provides an instructive opportunity for observing how the perceptions of the

formulation in terms of *folk* and *urban*, Tönnies' typology has provided a focus for American anthropologists and sociologists in their work on problems of social change. Perhaps of the most general significance, however, are Tönnies' basic statements on the use of "normal concepts" in empirical analyses, from which Max Weber's "ideal-typical method" received its impetus.

On the other hand, it seems also that some of Tönnies' assumptions diverted attention from various problems implicit in his subject-matter.

For example, by denying the "social system" quality of a *Gesellschaft*, he precluded an examination of integrative factors operative in *Gesellschaft*. Thus, he set the stage for the excessive "disorganization" emphasis which plagued urban sociology for some time.

Moreover, in his discussion of two subjective dispositions to act—designated by the translator as *natural will* and *rational will*—Tönnies seemingly treated these predispositions as determinants of *Gemeinschaft* and *Gesellschaft*. Although he actually assumed a reciprocal relationship between social and individual factors, his mode of analysis creates a strong impression of psychological reductionism.

As a result, the book has kept attention away from two important problem areas. On the sociological side, it delayed the concern with social facts in their influence on other social facts. On the sociopsychological side, it interfered with the formulation of problems regarding the influence of the social group on the individual, reversing what seems to be Tönnies' central hypothesis.

Finally, it must be said that a more specific obstacle to a thoroughgoing sociological analysis was the explanation of all *Gemeinschaft* phenomena in terms of *natural will*. It prevented Tönnies—despite his extensive knowledge of Marx—from raising questions as to the bases of support for the preservation of *Gemeinschaft* ideologies in a social system moving toward *Gesellschaft*. More basically, it reduced the notion of "equilibrium," which he recognized as a property of any system, to merely tangential significance in his book, keeping this idea from developing into a functional theory of social systems.

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Of What Is Intelligence Made?

Pierre Oléron

Les composantes de l'intelligence: d'après les recherches factorielles. Paris: Presses Universitaires de France, 1957. Pp. xiii + 517. 1800 fr.

Reviewed by LUCY RAU

Dr. Rau, trained at the University of California in Berkeley, partly in work on perception and cognition but presumably more as Chief Clinical Psychologist of a hospital's Child Guidance Service, is now at Stanford University, where Terman revised Binet and kept track of gifted children throughout the years. She is Assistant Professor of Psychology and teaches a course about the exceptional child and laboratory courses in mental testing.

THE study of intelligence has long exemplified an area of investigation in which theoretical preoccupations, developing methodology and practical application have been combined in a particularly fruitful way. Questions about the nature and structural interrelationship of intellectual abilities have been of central concern. The development of correlational techniques, and of factor analysis in particular, has provided a promising means for exploring these questions, and factorial studies of intelligence now form an imposing body of research. Now M. Oléron has provided us with a lucid, scholarly, and comprehensive review of these many studies. His book is a valuable contribution to a field where aspirations to solid empiricism and methodological rigor have sometimes been beclouded by name-magic and the heat of controversy. It is to be hoped that this very useful volume will shortly be made more accessible to American psychologists by translation.

This is not a book about the technique of factor analysis, and it makes no contribution to methodology as such. It does, however, review thoroughly and comprehensively the results of the fac-

torial studies of intelligence, and the clarity with which this mass of material is presented should make the account a valuable reference work. The subject-matter is approached from the point of view of historical development and the delineation of major theoretical issues. In Binet's work, the author traces the emergence of the study of intelligence from the older psychologies of faculties and sensations, an emergence under the impact of practical objectives. He gives in this connection an interesting discussion of the philosophical background of Binet's work, which may not be very familiar to most American readers.

The major controversy which has dominated this field, one which occupies the bulk of the book, is, of course, that between the Spearmanian two-factor or *g* theory of intelligence and the group or multifactor conception of Thurstone and others. The arguments of both sides, and the methodological developments and empirical findings which accompanied them, are presented in detail. M. Oléron's epistemological position seems to be solidly grounded in logical positivism, and this orientation lends a refreshing clarity to his discussion of these issues. Tolerantly he chides Spearman and some of the other combatants for their doctrinaire belligerence and succeeds in resolving a good many of the apparent differences between them. He demonstrates, for instance, that the disappearance of *g* in the multifactorial analyses is a direct product of the arbitrary requirement of simple structure which Thurstone imposes. On the other hand, he shows that Thurstone's method of analysis is a more refined and general one, of which Spearman's method

of tetrad-differences is shown to be a special case. This sort of clarification goes far towards organizing the results of factorial studies into a usable body of knowledge.

IF M. Oléron himself shows any bias, it is towards a pragmatic empiricism. Such bias would seem quite natural in a psychologist who is best known for his work on the measurement of intelligence in the deaf. This practical, empiricist orientation is evident in the discussion of the Spearman-Thompson controversy over *g*. The author decides that Thompson's arguments are inferior because they are purely formal ones; Spearman's proofs are based on empirical data. The pragmatic point of view is also obvious in the author's discussion of the essential dilemma of the factor analyst: the factors which have predictive significance usually are not 'pure,' whereas the pure factors are not predictive. In this quandary, prediction wins out.

On the whole, however, this is an impartial as well as hard-headed account of a field which has constantly been dogged by the specter of reification or accusations thereof. M. Oléron's broad impartiality exposes the reader to all of the complexities of fact and controversy and to a number of paradoxes. A striking example of the latter is Burt's rigorously nominalist position in 1938 as to the nature of factors (they are "only mathematical abstractions"). We find the same author today (see the recent paper in the *American Psychologist*, Jan. 1958, 13, 1-15) arguing vigorously for the results of factorial studies as proof of the strictly genetic nature of intelligence.

In his concluding chapter, the author points up what seems to be the ultimate limitation of the factorial method of studying intelligence. These studies are all based on the scores of individuals on a wide variety of tests. The test scores are products of the psychological processes or operations of the subjects, yet yield very limited information about those processes. Mental tests and the factorial studies based on them have made a very substantial contribution to our knowledge of intellectual abilities. We know that individuals dif-

fer reliably on a general dimension which we tend to think of as intelligence, and that these differences may be relatively stable over long periods of time. We also know that intellectual abilities are differentiated to a considerable extent and seem to become increasingly so with age. It has also been evident, since the very first non-unifactor studies of Webb and Cattell, that the distinction between intelligence and variables that we tend to think of under the rubrics of motivation or personality is difficult to maintain with any degree of sharpness.

All of this argument suggests to this

reviewer that a different research approach is now in order, to further our understanding of intellectual functioning. We can perhaps develop methods, whether experimental or observational, which will give us a closer look at the psychological processes involved in intelligence than do mental test scores. Developmental studies of reasoning and other intellectual functions (Piaget's work is an important pioneering attempt) would seem to be particularly important. The factorial studies have given us something resembling an aerial chart of the region. Perhaps now we can venture to send in some expeditions.

necessarily in the ways that have just been suggested. The effect has been mutual. Theories of behavior that have their roots in therapists' observations of their patients or clients have had an impact on general behavior theory and research. Thus, we can find the Blacky Test, a clinical instrument, wedded to the tachistoscope, a favorite laboratory tool, in research designs that reflect an amalgam of cognitive theories of perception and well-scrubbed versions of psychoanalytic concepts of repression. At the same time this eagerness to tame clinical psychology has led to some hasty and therefore ill-adapted attempts to extend the results of laboratory research to the clinic.

This, then, is the current state of clinical psychology. On the professional side it has expanded until it has overrun all spheres of group and individual relations in school, hospital, industry, and the community at large. On the academic side, it has been fought over by both clinical and academic theorists and by the growing group who strive to work out new integrations of these two streams of thought.

An Academic Clinical Psychology

John M. Hadley

Clinical and Counseling Psychology. New York: Alfred A. Knopf, 1958.

Pp. xv + 682 + xix. \$8.95 (trade); \$6.75 (text).

Reviewed by EDWARD S. BORDIN

Dr. Bordin is Professor of Psychology and Chief of the Student Counseling Division at the University of Michigan, where he has been coordinating training in clinical psychology for the past eight years. He is a past-president of the Division of Counseling Psychology of the American Psychological Association and editor-elect of the Journal of Consulting Psychology. He is the author of Psychological Counseling (CP, Nov. 1956, 1, 325-327) and reviewed Wallen's Clinical Psychology in CP (Feb. 1957, 2, 246f.).

THE events of World War II freed clinical psychology from the narrow confines of the diagnostic role it had assumed between the two great wars, and the forces thus released have not yet been tamed. Clinical psychology, like the newly emancipated, over-restricted, over-protected adolescent, has rushed to embrace many new experiences, like intensive psychotherapy in hospitals, clinics, or private practice, participation in social and medical research, industrial consultation, and medical administration. Society has become

so avid for these services that the number of clinical psychologists is expanding with such rapidity that other psychologists have become uneasy about the consequences of this growth for organizational structure and function of the American Psychological Association.

It is natural to look for ways to harness all this energy. How can we insure that clinical psychology is a part of psychology, yet not all of it? One way, chosen by many, is to insist upon the intimate relationship of general psychological theory and of laboratory research to the tasks of the clinical psychologist. This position draws strength from the intrinsic truth that all human behavior, whether in the laboratory or the clinic, must, by being human, bow to the same principles. It is supported by the widely held distinction (dogma?) between what is basic and what is applied. Clinical and other applied psychologists may outnumber the rest, but the minority are the keepers of the treasure chests.

The wider scope of clinical psychology has in fact revitalized its relation to basic theory and research, but not

ALL of this is caught by Hadley in his book on clinical and counseling psychology. The author's professional career mirrors these varied currents and is in turn reflected when he sets out to offer a preview of the profession intended for the introductory course "in clinical and/or counseling psychology." He can claim interest and experience as an experimental psychophysicist, a speech pathologist, a school psychologist, a communications specialist, and a classification specialist. He is currently a teacher and administrator of training programs in clinical, counseling, and school psychology.

Like Wallen, whose introductory text was published last year, Hadley makes psychotherapy or counseling—he prefers the latter term because it has its roots in psychology and education—the focal point of his book. The first two parts of this four-part volume are devoted to psychological theory as it is applied to psychological counseling and to practical procedures and techniques. Despite considerable use of concrete examples,

his exposition and discussion of therapy seem superficial and academic. Perhaps this impression is created by the extensive array of theories that he reviews. He finds five major issues in psychotherapy and analyzes the ways in which different theories treat them. These five major issues are: the role of insight, emotional release and tension reduction, supportive relationships and activities, relearning, and socialization. The theories considered run the gamut from Freud to his disassociates such as Lewin, Tolman, Rogers, Rotter, and Kelly.

Considering that this is a text for students just entering training, the treatment of therapeutic theory seems inappropriately ambitious. The limitation of space—perhaps also an awareness of the limited sophistication of his intended audience—seems to force upon the text a superficiality coupled with the posing of issues too complex for the space allotted. It is hard to see how students can avoid being alternatively lulled into an acceptance of oversimplified versions of theories and bewildered by seemingly unresolvable contradictions.

HADLEY espouses a combination of client-centered and field theory, but it seemed to this reviewer that he is not greatly devoted to either. In respect of the former there are many places where he adopts positions incompatible with it or makes contradictory assertions. While discussing the qualifications of the clinician (pp. 572f.), he calls attention to respect for the client as a cornerstone of Rogers' philosophy and goes on to assert that the clinician must be free of motives to control his clients by directing them or prescribing for them. Yet soon after, while discussing the scope of professional psychology (p. 602f.), he emphasizes the need for offering recommendations (naturally, only those fully qualified should do so) and for preliminary diagnosis. Similarly, he never really comes to grips with the implications for diagnosis of the assumptions of client-centered therapy. A quotation from Rogers (p. 310), stating his position that diagnosis interferes with treatment, he fails to meet head on. Instead, he deflects the issue toward how one can



JOHN M. HADLEY (right) counseling a counselee

gather the most meaningful information. He adheres to field theory, but only in its most general terms, holding that we must be interested in possible modification of the forces impinging on the individual (his environment) in addition to the possible modification of his attitudes or motivations. An eclectic empiricism is perhaps a proper description of the position taken.

After Hadley moves from counseling to evaluation and assessment, and then on to professional issues, the treatment becomes less academic. Part IV, which deals with professional issues, is the section that seems most unequivocally useful. Too few students are being informed and interested in the professional problems of psychologists.

The discussions of tests cover a wide range in considerable detail. Yet the detail is not great enough to provide the necessary background preliminary to the use of such devices as the Rorschach or the Minnesota Multiphasic. This is always a thorny issue in such books. Further, while discussing the validity of tests, Hadley throws empiricism overboard. The failure to find empirical verification for pattern interpretations of the Wechsler-Bellevue he sweeps under the rug. We are told that the best evidence for validity will come from the clinician's experience (p. 429). Tests cannot be evaluated apart from the clinician, but this evaluation seems to come down to "the kinds of information provided and the responsiveness of the client rather than a statistical index of validity or reliability" (p. 497).

In more ways than one would expect of a book that introduces and surveys a broad field, this volume is a personal document. Constantly it reflects its author's range of experience. One might add that it probably offers the broadest coverage of all recent texts. As the author ranges over various topics and describes various settings for clinical psychology, the reader finds himself becoming more and more acquainted with the author's vita. When he discusses the anamnesis, the forms he uses at the Purdue University Psychological Clinic, his present affiliation, are presented and, it must be added, discussed in almost wearisome detail. When George Kelly's theoretical position is taken up, it is Kelly, circa 1940, of Ft. Hays Kansas State College. Hadley took his undergraduate degree at that college. When the diagnostic interview is considered, one of the main sources is a manual on speech correction by Wendell Johnson of Iowa and two associates. Hadley is an Iowa PhD.

A MORE direct insertion of personal history induced a pronounced chuckle. By page 572 it had become very evident that the author felt strongly about treating clients as persons, not as cases. In fact, this admonition had been repeated so often that there was no forgetting it. One wondered whom he was fighting. Then comes the answer: "We once failed an examination in speech pathology because our entire paper made reference to speech defects instead of *persons with speech defects*." Clinicians will be clinical even while reading books by other clinicians about clinical psychology!

It should be clear that there are faults in this book. Still it provides a good panoramic view of clinical psychology. Perhaps, we should concede that many of its faults are to be charged to the field rather than to the author.



To study the phenomena of disease without books is to sail an uncharted sea, while to study books without patients is not to go to sea at all.

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Culture Theories: Their Truth and Use

Theodore Brameld

Cultural Foundations of Education: An Interdisciplinary Exploration. New York: Harper & Brothers, 1957. Pp. xxi + 330. \$5.00.

Reviewed by HARRY S. BROUDY

who is Professor of Education at the University of Illinois. He has published *Building a Philosophy of Education* (Prentice-Hall, 1954) and, with E. L. Freeland, *Psychology for General Education* (Longmans, Green, 1956). He has been president of the Philosophy of Education Society and also of the Association for Realistic Philosophy. Just now he is working on education's esthetic foundations.

THEODORE BRAMELD is Professor of the Philosophy of Education at New York University and for many years has advocated the view called *Reconstructionism*. In other words, he favors the use of education to reconstruct the social order in the direction of greater democracy. This orientation serves to unify the book but also constitutes one of its limitations.

From three-fifths to four-fifths of the volume is devoted to a critical and acute examination of the culture theories of Kroeber, Bidney, Kluckhohn, Sapir, Benedict, Boas, White, Herskovits, and others. The reader will learn wherein these theories agree, disagree, are and are not consistent with themselves or their presuppositions. What he will not find out, if he does not already know, is what these theories are about or how they were arrived at. The same comment can be made about the author's treatment of personality and learning theories of Fromm, Freud, Sullivan, Murray, Murphy, and others (Chapter 9).

To put it in another way, this is not a first book in anthropology, or learn-

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ing theory, or education, but, for those who know their way around in these disciplines, Brameld's treatment is always illuminating and sometimes brilliantly so.

The book does not claim to be an exhaustive interpretation of culture theory. It is rather a thorough examination of what educational theory and practice might look like when viewed with regard to the categories of cultural order, process, and goals. Inasmuch as these categories name three great problems common to both education and culture theory, they furnish the author with a convenient and instructive structure for organizing the vast array of materials from both fields. What such a structure does not give is a systematic treatment of any particular anthropological or educational system or thinker as such. The most valuable contribution of Professor Brameld lies in this unification which, although interwoven with the culture theory in each chapter, is recapitulated in Part V.

THE quarrel with the book—for those who may have a quarrel with it—will probably arise from the uneasy feeling that the selection of the topics, their treatment, and the educational conclusions derived from them are all influenced by the author's prior commitments to certain theories about the nature of a democratic society and the educational means of achieving it.

For example, Professor Brameld devotes considerable space to the *sui generis* theory of cultural reality, viz., that culture constitutes a superorganic mode of reality with its own laws and modes of explanation. Alfred L. Kroeber espoused this view but, to Professor Brameld's satisfaction, later recanted in favor of a more operational outlook. We find the following passage:

If one holds a *sui generis* view of the reality of culture, then one is likely also to hold that education can accomplish little except to conform with and endorse already given culture-configurations. If, however, one holds to an operational view, then one may more plausibly contend that education can articulate and even act upon the problems generated by these configurations (p. 82).

ON PROBLEM SOLVING

by

KARL DUNCKER

In the monograph *On Problem Solving* an investigation of the practical and mathematical problems in thinking is made. In its 112 pages are included the broad discussion areas of "The Structure and Dynamics of Problem-Solving Processes," "Insight, Learning and Simple Finding," and "Fixedness of Thought-Material."

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The quoted passage lends support to an argument of this form: My preferred educational scheme X is more consonant with culture or personality theory A than with culture or personality theory B. Theory A is therefore to be preferred to theory B, and because A supports X, educational scheme X is to be preferred to educational scheme Y. If Professor Brameld does not explicitly use this argument, others may, and to those who do, it may be pointed out that, unless we can establish the probability of these theories independently, they can draw little evidential nourishment from each other.

OF course, it may be that the present status of culture theory does not provide such independent validation. The sharp disagreement as to the meanings and implications of class structure (p. 78) is a case in point, leading Professor Brameld to observe:

It is legitimate to inquire whether educators are entitled to use the Warner definitions and criteria as uncritically as many have done (p. 80).

The question arises as to whether educators may not have much to learn from culture theory, and the answer is that they certainly have. The book will have served a noble end if it drives this point home. What is not so clear is that familiarity with this field brings with it any necessary consequences for educational strategy or curriculum design.

Certainly it does not seem to follow that due consideration of cultural dynamics entails a curriculum that devotes itself almost exclusively to this consideration.

Nor does it follow that because cultural process is ubiquitous and important, it must be treated as a seamless web for purposes of instruction. Professor Brameld deplores the fragmentation of current schooling into separate subjects, but one cannot understand a seamless web; one merely becomes entangled in it. Perhaps the crystallization of knowledge into distinguishable disciplines is culture's way of becoming intelligible to its members. And while Professor Brameld's emphasis on interdisciplinary study should elicit a universal *Amen*, before there can be inter-

disciplines, there must first have been disciplines.

Finally, though culture theory may purport to explain cultures as they have existed and developed, still they may give no norm for what the school *ought* to be in any given culture. From Warner's theory about the class structure of

American society, what follows about the desirability of having American schools 'biased' in favor of middle-class values?

Yet the book has genuine merits.

These logical precautions, it is hoped, will help to reveal them, rather than serve to dim them.

A Slightly British Systematic Psychopathology

D. Russell Davis

An Introduction to Psychopathology. New York: Oxford University Press, 1957. Pp. vi + 388. \$7.50.

Reviewed by M. ERIK WRIGHT

Dr. Wright is Professor of Clinical Psychology and Psychiatry at the University of Kansas and Director of the Clinical Psychology Program and Services at that university. He has a long history of practical experience in helping people who are in trouble, first at state hospitals, reform schools, institutions for the mentally retarded, and epileptic colonies, and then later in private practice. He received his formal training to a clinical PhD with a firm grounding in systematic psychology at the State University of Iowa when Lewin, the elder Seashore, Malamud, Lee Travis, Ruckmick, and Stoddard were there. Later the University of California gave him an MD. He likes to work with somatopsychology, psychosomatology, and rehabilitation, and is also especially interested in the nature of hypnosis and hypnotic therapy.

"My purpose is . . . to build a bridge between the psychiatric clinic and the psychological laboratory." This introductory comment describes not only the goal of the book but also the objectives of Dr. Davis' training and experience. He had judged his psychiatric education to be seriously lacking in the study of psychological research methods, especially the laboratory approach. Thus, when presented with the opportunity to work with Pro-

fessor Frederic Bartlett at Cambridge University, he eagerly grasped at it. The Second World War and other circumstances extended his originally more limited plan of association with this group of research psychologists until 1948, when he returned to the private practice of psychiatry, though still maintaining his academic contact via a lectureship in Psychopathology at Cambridge.

The main thesis of this book is the belief that mental disorders and behavior deviations can best be understood through psychological theories and psychological research. Such an exposition the author considers necessary in view of the continued and strong disposition in British psychiatry to seek for basic explanations in genetic, biochemical, and constitutional factors.

Psychopathology is defined as that branch of psychology (an area of biological science) applied to a certain range of human problems. Dr. Davis' conception of biology is that it is a comprehensive study of life phenomena and that it includes cultural anthropology, ecology, and the broad realm of environmental factors usually studied by social scientists. He clearly does not intend an evaluation of all psychological theories which might have relevance to psychopathology, but concerns himself selectively with a presentation of some



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John Paul Scott is senior staff scientist and chairman, Division of Behavior Studies, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine. His *Animal Behavior* was published last year by the University of Chicago Press.

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1958 312 pages \$10.00

WHAT THE REVIEWERS THINK ABOUT FRENCH'S THE INTEGRATION OF BEHAVIOR

In a critical book review in the *American Journal of Psychiatry*, Dr. H. K. Johnson evaluated *THE INTEGRATION OF BEHAVIOR*, Volume I: Though he admits to a deep-rooted skepticism, here is the way he concluded his review:

"... (the secret is out; this reviewer is a skeptic) should not detract from the fact that 'Basic Postulates' is a very important book, that it represents years of thought and work, and reflects an author of serious purpose and integrity... Perhaps his theory of psychological absorption, his emphasis upon play, and his concept of the ego as a guiding integrative field, among others, are permanent contributions. Possibly the only criticism, and it is hardly that, is that

he is ahead of his age. Here he would resemble Leonardo da Vinci—and possibly Freud... Dr. French has set his sights high... One can only wish him well in his courageous, and one suspects, lonesome scientific Odyssey."

Joseph Zinkin, writing in the *Journal of Nervous and Mental Disease* commented:

"... highly instructive, clearly stated, and carefully worked out. It requires a close and serious reading, and must be considered a major attempt to build the foundations of a thorough-going ego psychology on the basis of a few very fundamental but sound principles... This book is not meant as a species of partisan pleading."

In *Psychosomatic Medicine*, Elizabeth Zetzel wrote:

"Dr. French is to be congratulated on undertaking this very ambitious project. There is no question that he has much to offer, particularly in his approach to the detailed and patient understanding of individual dreams."

Theodore Lidz, in a critical appraisal in the *American Journal of Psychiatry* said of Volume II:

"Taken by itself, the book is of interest in providing an opportunity to follow the most intensive study ever published of a patient's dreams..."



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basic psychobiological concepts and a consideration of those psychological research methods that might serve to correlate concept and observable behavior and thus hopefully lead to hypotheses that can be subjected to operational testing (i.e., to behavior which could be described objectively).

A substantial portion of the text (five out of 14 chapters) is devoted to the effects of the family upon the individual's behavior. The presentation of the effects of these environmental factors gave the reviewer the impression of condensed and oversimplified versions of textbooks in child psychology, developmental psychology, and experimental psychology, incorporated into this larger work on psychopathology. This sincere effort to present parsimoniously significant issues (e.g., nature-nurture, IQ constancy) and then to resolve them succinctly, poses the author a very difficult problem when dealing with such complex issues. His conclusions are often open to considerable question.

DR. DAVIS does not appear to be impressed with many of the psychoanalytic hypotheses about behavior deviations. He feels that any psychological theory can make use of certain general dynamic concepts (e.g., repression, pleasure principle) without a commitment that accepts as valid or useful other psychoanalytic concepts. The most productive psychological approach to psychopathology lies, he believes, in some kind of behavioral learning theory, one not too divergent from the general model proposed by O. H. Mowrer (1950). He finds in concepts, like drive-strength, conditioning, re-enforcement, and extinction, the significant explanatory principles which provide a fruitful scientific and clinical basis for correlating learning experience with behavioral disturbances. These concepts are, however, but loosely defined in the text, and one could, perhaps, readily substitute the concepts of association theory or of other motivation-oriented systems without seriously disturbing the logic of the presentation. The chapters devoted to the implications of experimental neurosis for psychopathology, the application of behavior theories to psychopa-

thology, and the disorders of mental function are well written, and the correlations between conditioning learning behavior theory and behavioral deviation are interestingly presented.

Although the author shows a broad familiarity with psychological research, there are some areas which suggest a limited contact with the literature. For example, the discussion of hypnosis and psychopathology repeats the unsubstantiated platitudes disparaging the contributions of hypnosis to psychotherapy: "Hypnosis and suggestion do nothing to change the conditions responsible for the appearance of symptoms. Nor do they help to elucidate those origins of illness which lie in the past experiences of the patients" (p. 27). (A few pages later Freud is described as impressed with the exceptional accuracy and distinctness of the experiences which his patients recalled in the hypnotic state.)

Another instance is the author's treatment of the "content of mental activity." When this material is obtained from psychotherapeutic interviews, he considers it to be of significance in the understanding of etiology, but he dismisses summarily the research with projective techniques—the Rorschach and the TAT—as of minor importance because they reveal "only the content of mental activity."

The author seems to have a low level of confidence in the research methods and the data that social psychologists, anthropologists, and sociologists have gathered. He also doubts whether these areas might ever make any significant contribution to psychopathology. It is hard to reconcile the obvious importance he assigns to environmental factors with this depressed and discouraged evaluation of what these social sciences have to offer to the study of abnormal human behavior.

There is a much more systematic point of view in this book than one finds in such comparable texts as O'Kelley and Muckler (1955) and Yacorzynski (1951). This volume could be read profitably by a student for an introductory course in psychopathology or by anyone interested in the correlation of this particular kind of conditioning-learning theory of behavior to the phenomena of psychopathology.

Neurology Gently Revised

Ernest Gardner

Fundamentals of Neurology. (3rd ed.) Philadelphia: W. B. Saunders, 1958. Pp. xi + 388.

Reviewed by LEON S. OTIS

who is Assistant Professor of Psychology at the Johns Hopkins University, where he teaches courses in physiological and comparative psychology. Right now he is busy studying how early fear (anxiety) may, by conditioning, affect the later behavior of an organism.

THE principal changes in the third edition consist of a new chapter entitled, *Chemistry of the Nervous System*, twelve additional illustrations, and a somewhat more complete and up-to-date reference list appended to each chapter. There are in all twenty-nine additional pages.

The new chapter follows the approach and style set by the other chapters and, indeed, by the first two editions. It presents a few of the basic concepts and, as the author puts it, "a brief summary of some major trends and problems." In ten pages he offers a resumé of the high lights of general and intermediary metabolism, a few paragraphs on the chemical composition of nerve cells, and a few lines on the behavioral effects of so-called hallucinogens and tranquilizers.

Very little in the way of revised or new material is seen in the nineteen chapters that are shared by both the second and third editions. With the exception of some paraphrasing and a few lines of fresh material here and there, the text of the two editions is almost identical.

A new edition must meet certain minimum criteria in order to be acted upon favorably by the copyright office, and this third edition is, indeed, copyrighted. This reviewer thinks, however, that the copyright laws must be very lenient.

ON THE OTHER HAND



RESEARCH ON DISASTER

Dr. Bushnell's review of four disaster studies (*CP*, July 1958, 3, 200-202) is—aside from the flip title—constructive and helpful.

It is true, as he says, that more disaster research during the past several years has had a sociological or social psychological orientation. However, the interest and production have not been nearly so lopsided as he implies. Two of the earliest publications in the Disaster Study series (*Tornado in Worcester*, by Wallace and *The Child and His Family in Disaster*, by Perry, Silber, and Bloch) would seem to be about what Dr. Bushnell ordered. In *Tornado in Worcester* and in articles and papers, Anthony F. C. Wallace (a member of the Committee on Disaster Studies) has analyzed disaster behavior in terms of the individual's reactions to his perception of the destruction of his culture. Dr. Bushnell mentions *Disaster: A Psychological Essay*, by Martha Wolfenstein. This book is what the subtitle says it is and it deals intensively at several points with the relationship between culture and personality in disaster. Consultation of the articles and the bibliography in the Summer (1957) issue of *Human Organization* will reveal other examples. The literature, including that which had some sponsorship from the Committee on Disaster Studies, contains much more psychological and anthropological work and more theoretical work on human behavior in disaster than Dr. Bushnell's review implies. At least three monographs which we will publish within the next year, in addition to the work of Dr. Chapman which he mentioned, will deal explicitly with psychological variables and one of them directly with culture-personality relationships. One is a theoretical monograph. His own analysis of the Hupa Indians in disaster will, of course, provide additional analysis in the culture-personality area and will, I hope, further stimulate the interest of our anthropological colleagues.

In connection with Dr. Bushnell's main plea, it is amusing to note that a group of sociologists took the opposite position in an article in the *American Sociological Review*. They felt that disaster research had

been too much dominated by psychological and psychiatric points of view and insisted that the sociological and social psychological points of view had been neglected! (William H. Form, Charles P. Loomis, *et al.*, The persistence and emergence of social and cultural systems in disaster, *Amer. Sociol. Rev.*, April 1956, 21, 180-185.)

I welcome and join in Dr. Bushnell's plea for more "anthropological and personality oriented footwork" in disaster research, if by *footwork* he means solid, well-designed research projects. This plea should be addressed to our colleagues in the behavioral sciences, however, rather than to the Committee on Disaster Studies. In the first place, there is no longer a Committee on Disaster Studies. It has been succeeded by the Disaster Research Group which carries on its own program of research and research interpretation and which is not a grant-making organization. More importantly, it would be most unfortunate to assume that responsibility for the development of this, or any other field of research, lies primarily with any organization. Organization can support, stimulate, and in the early stages guide research in a new field. But, if the objective is a field of scientific inquiry, rather than a particular research project or program, the ultimate responsibility for interest, selection of topics, conduct of research, solution of methodological problems, and development of theory lies, and must lie, in the scientific community itself.

If behavioral scientists become interested in research on human behavior in disaster, they should define these interests and design their research projects in the context of their own interests and competence—with, of course, a serious effort to become familiar with the preceding work. There are various sources of support to which potential investigators can turn and there is now a literature from which scientific interests and research designs might grow. While the Disaster Research Group is willing and pleased to advise with colleagues about disaster research, there is no intrinsic need for any middleman between the investigator and the potential supporter of his research. The only point upon which I

would suggest that 'coordination' is essential is this: it would be well for any investigator to check with us to see if we know of any other group which is planning to make a study in the same disaster-stricken community. Lack of very careful coordination at the site could very easily result in spoiling both studies.

HARRY B. WILLIAMS
Technical Director, Disaster
Research Group
National Academy of Sciences-
National Research Council

IN APHORISMS NO NONSENSE

CP SPEAKS of May 1958 (*CP*, 3, 125f.) quite misrepresents my views. My point is that *CP* should be prepared to defend its quotations as *non-nonsensical*, which is not the same thing as defending their *truth* or *validity*. Most of the quotes with which *CP* closes its gaps strike me as quite sensible and plausible, and unobjectionable whether or not I happen to agree with the thought expressed. When, however, *CP* comes a cropper logically, as in the quote from C. P. Steinmetz, the "electrical wizard" (*CP*, Aug. 1957, 2, 216), I cannot help noting its fall. That Steinmetz' statement is formally absurd or self-contradictory is evident to anyone who considers its reproduction below (parcelled out into numbered propositions but otherwise unchanged from *CP*'s quote).

1. Mathematics is the most exact science, and
2. its conclusions are capable of absolute proof.
3. But this is so only because mathematics does not attempt to draw conclusions.
4. All mathematical truths are relative, conditional.

In simple logic, propositions 2 and 3, and 2 and 4 are mutually exclusive and cannot be simultaneously held without weaseling of key terms.

Would *CP* undertake to justify this kind of witless aphorism under the rubric "posterous assertion by a great man"? If so, the attempt strikes me as both late and lame. And how does sermonizing about "stimulating thought" and not undertaking to "disseminate truth" and the perils of "stuffy authoritarianism" answer the criticism directed at *CP* for filling its niches with *undrawn conclusions* and *relative absolutes*? Please explain.

Is it "that Homer nods" or "we who dream"?

RAYMOND J. MCCALL
Marquette University

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THE CASE OF JIM

Two reviews of *The Case of Jim*, a phonograph record of counseling, have appeared in *CP*. The review by Manoil (*CP*, Mar. 1958, 3, 78) seemed to capture the spirit and purpose of the record. Mahan, on the other hand (*CP*, May 1958, 3, 142), found nothing of value in the record. In his review there are several matters which warrant comment.

Concerning the client, Mahan objects that "the subject is an emotionally overwrought person of limited intelligence whose oral productions can only be described as distressing to the listener." One gets the feeling from this comment that in order to qualify for therapy a client should be relaxed, well-behaved, and bright.

It may be of incidental interest to note Mahan's accuracy of estimate with regard to the client's intellectual capacity. The client's "limited intelligence" enabled him not only to graduate from college but to go on to secure a Master's degree.

Mahan comments as follows about the ending of therapy: "The recording ends on the same depressing level which it showed in the beginning, and it left this listener with a marked feeling of futility." One can only conclude that the reviewer considered the case to be a total failure. Judgments about therapeutic outcome are strikingly complex judgments to make, and I would not venture any categorical assertions about the outcome of this case. Yet I should like to record the view that I do not feel nearly as depressed about the outcome as did Mahan.

But this is almost beside the point. What seemed most striking and regrettable about the review was its failure to come to grips with the record's essential purpose. That purpose was not to present a "successful case." The purpose was, rather, to present the direct interaction of counselor and client—the elemental data of the counseling process—and to explore some of the meanings of this interaction for the more general understanding of personality. It was unfortunate to have the review bypass the chief reasons for the record's existence.

JULIUS SEEMAN
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The problem of a coherent civilization is the problem of living with ignorance and not being frustrated by it.

—J. ROBERT OPPENHEIMER



Films

Mental Health

Mental illness in its individual dimensions requires specialized diagnosis and treatment, which presuppose training and educational facilities for the training of such competent personnel as psychiatrists, clinical psychologists, social workers, nurses, and psychiatric aids. Treatment of mental illness, moreover, requires appropriate hospital facilities in terms of buildings, drugs, and necessary equipment.

Considering the great number of mentally ill persons, the quantity of personnel, buildings, and equipment necessary imposes a social burden that affects great numbers of people. Mental illness, therefore, should be considered also in its social dimensions; that is to say, the solution of the problems of mental health requires the participation of more and more people, not only as specialized personnel, but also as simple taxpayers or voluntary contributors. To achieve appropriate cooperation of the public in general a better understanding of mental illness in all its aspects becomes a basic requirement. In this way, problems of mental health become essentially educational problems. The public has to be provided with accurate information on the nature and extent of these problems and be made aware of their social nature.

The need for more education in this area has long been recognized and various civic and cultural groups have been at work in this field for many years. More recently, the 16-mm film and various TV programs are making remarkable educational contributions, either directly or in combination with other cultural and civic organizations.

The following film is a 16-mm kine-scope, originally presented on the *Arm-*

ADOLPH MANOIL
Film Editor

strong Circle Theater television program. It represents a recent contribution to more education in the area of mental health, with documentary material from the Cleveland State Hospital.

Man in Shadow

Produced by Smith, Kline and French Laboratories. 16-mm kinescope film, black and white, sound, 55 min., 1956. Available through Smith, Kline and French Laboratories, Medical Film Center, Philadelphia 1, Pennsylvania.

Mental illness as affecting the patient and his family are dramatically presented. The public's general attitude toward mental illness is still fraught with prejudices and fears, misunderstandings, and false ideas. The stigma attached to the psychiatric patient affects the behavior of his family and creates serious problems as to his care and rehabilitation.

Man in Shadow dramatizes the condition of the patient, the reaction of his family, the conditions of the mental State Hospitals and the need for a better understanding of the general problem of mental illness.

The reaction of the family is pictured through the strong opposition of the mother to the commitment of her son to a mental hospital. Her opposition is an example of prejudice, misunderstanding, and fear of public opinion. It epitomizes a general attitude toward mental illness, the lack of understanding of the needs of the patient, and a complete ignorance or misunderstanding of contemporary approaches to psychiatric problems.

The wife of the patient, contrary to the attitude of the mother, shows her willingness to cooperate by recognizing the problem and accepting the help of the psychiatrist.

The contrast of attitudes between the mother of the patient and his wife describes symbolically the distinction between enlightened approaches to mental illness and ignorance.

The patient himself is not seen in the film, but his voice and conception of the world are presented through the device of the TV camera. This includes the onset of the illness, his emotional responses to treatment, and especially his loss of contact with reality.

These 'reflected' reactions of the patient allow the presentation of characteristic conditions of the mental hospital with its lack of personnel, crowded conditions, and need of facilities for treatment. The problem of getting some contact with the patient, the use of insulin and electric shock therapy, occupational therapy, drugs, music, and group psychotherapy are also shown.

The film as a whole presents the problem of mental health as a complex system of interaction in which the family, the psychiatrist, the psychologist, the nurse, the hospital, and the public at large, all play an important role.

A solution to the problem would require a large scale educational program and continuous enlightened cooperation of private and governmental agencies.

The film is particularly intended for lay audiences, but it could also be used with classes in introductory or abnormal psychology as a ground for discussion and analysis of different aspects of mental illness.

The Key

Produced for the National Association for Mental Health by Campus Film Productions. Produced with the cooperation of the New York State Department of Mental Hygiene and the New Jersey State Department of Institutions and Agencies. 16-mm motion picture film, black and white, sound, 32 min., 1957. Available through National Association for Mental Health Film Library, and Contemporary Films, Inc., 13 East 37th Street, New York 16, New York. \$145.00.

The situation of the mentally ill is symbolized by the use of the key that could be used to open and close doors by oneself with freedom and confidence,

or to lock in patients as is the practice in many mental state hospitals.

The film begins by presenting the plight of a child left behind by his mother at the onset of her mental illness.

The patient, Myra Howard, is followed through her behavioral disturbances, and her treatment in a mental state hospital. This allows the presentation of hospital conditions, and of various methods of treatment including electroshock therapy, chemotherapy, and psychotherapy. The overcrowding of the state hospitals requires transfer of patients from wards for active treatment to wards for chronic patients where custodial care predominates. Lack of trained personnel and overcrowding do not allow helping the mentally ill to the degree made possible by contemporary psychiatric progress.

The situation in certain mental state hospitals is contrasted with the presentation of a modern, well-equipped psychiatric hospital, where the patients receive proper care through the use of the most advanced therapeutic techniques.

The second hospital emphasizes the value of milieu therapy as well as the importance of rehabilitation, follow-up, and prevention.

The film presents the general problem of mental illness both as an individual and as a social problem. Its adequate solution would require the enlightened participation of various segments of the population, presupposing a large scale educational effort that should provide information and appropriate understanding of the actual conditions of the mentally ill patient.

The problem presents a particularly significant aspect in a democracy, for at the present not all patients are given an equal chance at profiting from psychiatric progress.

The film, besides having a documentary and dramatic value, represents also a plea for cooperation and understanding of the needs of the mentally ill. It is well adapted for lay audiences and should contribute to creating a better appreciation of contemporary psychiatric progress and of social obligations toward the patient, and may be used for discussion in certain classes in psychology.

DAY HOSPITAL FOR PSYCHIATRIC PATIENTS

Back Into the Sun

National Film Board of Canada. Allan Memorial Institute of Psychiatry, Montreal. 16-mm, black and white, sound, 27 min., 1957. Available through McGraw-Hill, New York, \$125, and rental sources.

Reviewed by GEORGE M. GUTHRIE
The Pennsylvania State University

The National Film Board of Canada has earned a reputation for superior films on mental health. This production describes a new pattern of treatment being used at the Day Hospital of Allan Memorial Institute of Psychiatry of McGill University. The unusual feature is that patients spend nights and week ends at home. During the hours in the hospital a varied program of psychotherapy, recreation and medication is conducted as in any active psychiatric hospital. Returning home at night keeps the patient in his home and family and avoids subtle influences of being labeled and of labeling himself as a mental patient who was sent away.

Specifically, this film shows the story of a young woman. A limited picture is developed of her problems with her mother. The patient's difficulties with her husband and children are suggested. A short scene shows her gaining considerable insight in a sociodrama experience. But it is not the purpose of the film to show her illness, nor its causes, nor the detailed process of her recovery. Rather the purpose is to demonstrate the possibility of open-ward treatment of disturbed individuals without breaking their family continuity.

The film would be very useful for raising public support for a day hospital. In a more general vein, it shows that emotional disturbance will respond to careful treatment. As such, this film is most appropriate for intelligent but nonprofessional audiences. It would provide an excellent introduction to the topic of psychopathology in introductory courses where a variety of symptoms and treatments need to be shown. Most of all, it should achieve its purpose of creating the impression that those under a cloud can move back into the sun.

MISSIONARY MEDICAL WORK

Monanga

Produced by Smith, Kline and French Laboratories in cooperation with American Medical Association. 16-mm kinescope film, color, 55 min., 1956. Available through Smith, Kline and French Laboratories, Medical Film Center, Philadelphia 1, Pennsylvania.

The work of a medical missionary in Lotumbe, a remote village of the Belgian Congo, is presented.

Visits to the mission leprosarium and to a primitive semi-Pygmy tribe are also shown.

HOSPITALS FOR THE MENTALLY ILL

A World Alone

Eric Severeid, narrator. Produced by Smith, Kline and French Laboratories. 16-mm kinescope film, color, sound, 25 min., 1957. Available through Smith, Kline and French Laboratories, Medical Film Center, Philadelphia 1, Pennsylvania.

Shows conditions prevailing in mental hospitals. Present problems and plans for the future are also discussed.

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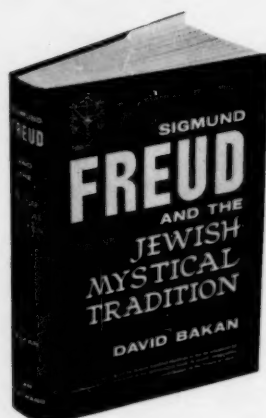
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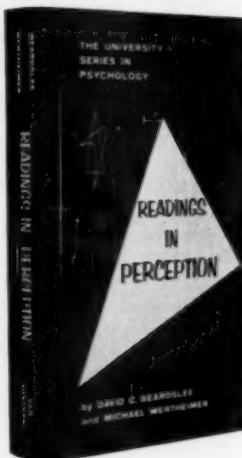
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